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The Planetary Theory of Newton

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I

The title under which Newton's chief work became famous in the history of human knowledge, "Philosophiae naturalis principia mathematica," suggesting as it does nothing astronomical, finds its explanation in the beginning of his third book. Having dealt with "the motion of bodies" in the first and second books, in the third he turns to astronomy in treating "the system of the world." Here he tells how first he had written it in a popular way in order that many should be able to read it; but afterwards, considering that those not sufficiently acquainted with the first principles, would not be able to understand the strength of the conclusions and would not give up their prejudices, and, wishing to prevent needless disputes, he chose to give it the mathematical form of propositions. Thus it can be read only by those who first have mastered the mathematical principles laid down in the first book. Not all the propositions demonstrated there will be needed, but only the first sections, the definitions and laws of motion, and such propositions as will be quoted afterwards.

Thus mathematics will be the foundation; mathematics applied to the forces and motions of the bodies. This is what we call the science of mechanics. In his preface to the first edition Newton himself introduces this name of rational mechanics, and he makes a highly instructive comparison with geometry. Practical mechanics, in the manual arts, works more or less imperfectly, just as practical geometry in drawing the lines is not perfect, whereas from olden times the use of geometry was to demonstrate and solve propositions. "Therefore geometry is founded in mechanical practice, and is nothing but that part of universal mechanics which accurately proposes and demonstrates the art of measuring. But since the manual arts are chiefly employed in the moving of bodies, it happens that geometry is commonly referred to their magnitude, and mechanics to their motion. In this sense rational mechanics will be the science of motions resulting from any forces whatsoever, and of the forces required to produce any motions, accurately proposed and demonstrated. . . I consider philosophy rather than arts and write not concerning manual but natural powers, and consider chiefly those things which relate to gravity, levity, elastic force, the resistance of fluids, and the like forces, whether at-

tractive or repulsive; and therefore I offer this work as the mathematical principles of philosophy, for the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena. . ."[1]

Rational mechanics was the discipline needed to unite earthly and celestial motions into one system. Earthly motions were ruled by Galileo's laws of falling and gravity, celestial motions were ruled by Kepler's laws of planetary orbits; there was no connection, no link between them. Gradually in the 17th century opinions were expressed that orbital motion was a kind of equilibrium between a centrifugal and a centripetal force. But such ideas could be fruitful only when cleared up by a rational system of fundamental concepts and rules. This was the first thing Newton had to do in his introductory "definitions" and "axioms, or laws of motion." Here he states the principles, since then appearing in every textbook, that every body, if not impelled by a force, continues in its state of rest or of uniform motion; that the change of motion, the acceleration, is proportional to the motive force; and that the mutual actions of two bodies upon each other are equal and contrarily directed. Whereas the first named principles easily proceed from what Galileo and his successors had found, the third one is explained at length to show how it is involved in all practical mechanics. Mass is introduced as quantity of matter "arising from its density and bulk conjointly," sometimes also simply called "the body"; "the quantity of motion arises from the celerity multiplied by the quantity of matter; and the motive force arises from the accelerative force multiplied by the same quantity of matter." Mass and weight are clearly distinguished. "Hence it is that near the surface of the earth where the accelerative gravity, or force productive of gravity, in all bodies is the same, the motive gravity or the weight is as the body; but if we should ascend to higher regions where the accelerative gravity is less, the weight would be equally diminished, and would always be as the product of the body, by the accelerative gravity."

Because the chief aim is the treatment of the freely moving heavenly bodies, here in the introduction centripetal forces are introduced "by which bodies are drawn or impelled, or any way bend, towards a point as to a centre." "Of this sort is gravity . . . and that force, whatever it is, by which the planets are continually drawn aside from the rectilinear motions, which otherwise they would pursue, and made to revolve in curvilinear orbits. . . They all endeavor to recede from the centres of their orbits; and were it not for the opposition of a contrary force which restrains them to, and detains them in their orbits, which I thereforce call centripetal, would fly off in right lines, with a uniform

[1] All the quotations are from the translation of F. Cajori, published Berkeley, 1934.

motion." Then, after mentioning a projectile shot from a mountain horizontally with sufficient velocity, which would go round the earth in an orbit, he proceeds: "the moon also, either by force of gravity, if it is endued with gravity, or by any other force that impels it towards the earth, may be continually drawn aside towards the earth, out of the rectilinear way which by its innate force it would pursue; and would be made to revolve in the orbit which it now describes; nor could the moon without some such force be retained in its orbit . . . it belongs to the mathematicians to find the force that may serve exactly to retain a body in a given orbit with a given velocity . . ." That he himself was this mathematician will appear later on.

Thus the edifice of rational mechanics was erected as the basis for an explanation of the heavenly motions. But this could not be done at the time without the simplifying notions of absolute time and absolute space. So he puts forward the theses: "Absolute, true, and mathematical time, of itself, and from its own nature, flows equally without relation to anything external. . . Absolute space, in its own nature, without relation to anything external, remains always similar and immovable. . . Place is a part of space which a body takes up, and is according to the space, either absolute or relative. . . Absolute motion is the translation of a body from one absolute place into another. . ." Newton was quite well aware that in common life (and in physical experiments as well) we always have to deal with relative places and relative motions. "It may be that there is no such thing as an equable motion, whereby time may be accurately measured. All motions may be accelerated and retarded, but the flowing of absolute time is not liable to any change." "But because the parts of space cannot be seen, or distinguished from one another by our senses, therefore in their stead we use sensible measures of them. . . And so instead of absolute places and motions, we use relative ones; and that without any inconvenience in common affairs; but in philosophical disquisitions, we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them. For it may be that there is no body really at rest, to which the places and motions of others may be referred."

So absolute time, space, and motion are philosophical conceptions; they are abstractions, nowhere to be found in experience, but nevertheless deduced from the facts of experience by the abstracting power of human mind. Newton considers them as the true reality behind the phenomena; modern natural philosophy considering "observables" as the only reality, has abandoned them entirely. But it must not be lost from view that in the 17th century, in the gradual elaboration of mechanics as the science of motion, this conception of "absolutes" was necessary in order to subject the phenomena to simple mathematics; only by expressing absolute motions in simple formulas the observed

relative motions could be deduced. The necessity of Newton's principles cannot be more stringently demonstrated than by the fact that only as late as two centuries afterwards—though relative place could be treated earlier already—the theory of relativity with its complicated algebra was able to handle relative time and relative motion in a satisfactory way. In astronomy, moreover, we have frames of reference the irregularities or the motions of which are so slight—the fixed stars, the rotation of the earth—that for practical use place, time, and motion relative to them could be considered as a sufficient approximation to absolute place, time, and motion, to suggest the conception of such absolutes.

Newton was able to lay the foundations of astronomy not only because, first, he was a renovator of mechanics, by elaborating its principles, but, secondly, because he was a renovator of mathematics too. The mathematics needed in these problems of curvilinear motion is the theory of fluxions, developed by himself at an earlier time already. In the *Principia*, it is true, no fluxions are found; it is all geometry. In some of the biographies it is said that he had worked out the problems and found their solutions by the method of fluxions, but that in order not to make the understanding too difficult by using an unfamiliar form of analysis he transferred them into the well-known forms of classic geometry. Perhaps it is more right to say that it is the spirit of the method of fluxions which pervades his geometry, the idea of considering quantities and motions not as definite values but in the process of originating, changing, or disappearing. This essence of the new fluxion method, our modern calculus, is most clearly elucidated in some sentences at the close of Section I: "by the ultimate ratio of evanescent quantities is to be understood the ratio of the quantities not before they vanish, nor afterwards, but with which they vanish. In like manner the first ratio of nascent quantities is that with which they begin to be. . . . For these ultimate ratios with which quantities vanish are not truly the ratios of ultimate quantities [called indivisibles], but limits towards which the ratio of quantities decreasing without limit do always converge; and to which they approach nearer than by any given difference. . . . Therefore if in what follows . . . I should happen to mention quantities as least, or evanescent, or ultimate, you are not to suppose that quantities of any determinate magnitude are meant, but such as are conceived to be always diminished without end."

Thus in his demonstrations Newton makes use of geometrical figures of straight lines, of triangles, rectangles of finite size; but then he lets the number of such parts be augmented and their size diminished *in infinitum*, to fit a curved orbit and a continually working force; and he shows that then the demonstration holds. It is for this reason that Book I opens with a first section on limiting properties and values in the case of evanescence of the quantities considered, and demonstrates

that the versed sines in curves lines are as the squares of the chords, the tangents, and the arcs, and that the spaces which a body describes by a finite force urging it are as the squares of the times.

II

The first proposition serving to build a bridge from Kepler's empirical laws of planetary motions to the acting forces deals with the law of areas. If a revolving body is subject to a centripetal force directed to a fixed point, the areas described by radii drawn to that point will be proportional to the times in which they are described. For the demonstration Newton makes use of finite time intervals, after each of which the force gives a finite impulse to the body towards the centre; so the

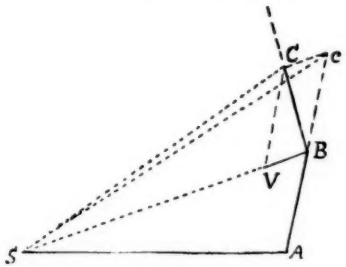

FIGURE 1

motion remains the same during an interval (from A to B in Figure 1) and then at B is changed suddenly by an additional motion BV; in the second interval, instead of continuing the motion along Bc = AB, the body follows the resulting path BC. Then, geometrically, the areas SAB and SBc are equal; the areas SBc and SBC are equal because the impulse BV was directed toward S; so the area SBC is equal to SAB. This holds for every further interval. Every next triangular area is equal to the preceding one, and they will lie in the same plane. "Now let the number of those triangles be augmented, and their breadth diminished *in infinitum;* and their ultimate perimeter will be a curved line." The impulses, having become continually smaller and more numerous, in the limiting case constitute a continually acting force, always directed to the centre S, and the areas remain proportional to the times.

In the same way the reverse is demonstrated. When a body moves in a plane curved orbit in such a way that the radii drawn to a point describe areas proportional to the times, it is urged by a centripetal force directed to that point. If in Figure 1 area SBC = area SAB, then area SBC = area SBc, hence Cc is parallel to BV, *i.e.,* the impulse by the force is directed along BS, toward the point S.

Thus Kepler's law of equal areas proves that the planets in their orbits are moved by a centripetal force directed to the sun.

Some further conclusions may be drawn. If in the limiting case a

continuous orbit is described through the points ABC in the figure, then drawing the chord AC it appears that the versed sine belonging to the arcs AB and BC is half the distance BV, hence proportional to the force in B. In the simple case of circular motions the versed sines are equal to the squares of the arcs divided by the diameter, hence the forces will be as the squares of the arcs described in the same time divided by the radii of the circles. Since the arc in unit time is the velocity, the equation derived by Huygens for the centrifugal force comes out: that the centripetal forces are as the squares of the velocities divided by the radii; or, as the radii divided by the squares of the periodic times. Kepler had discovered and expressed in his third law that the periodic times of the planets were as the 3/2th power of the distance. So the velocities, in the simplified case of circular orbits, vary inversely as the square roots of the radii, hence the forces acting upon such planets are as the inverse squares of the distance. Thus the law of centripetal force toward the sun is derived from Kepler's third law.

III

In a more general way, however, this law can be derived from the laws of the elliptic motion of a single planet. Newton first derives a general expression for the centripetal force in a curved orbit. The deviation of the body from the tangent, in the direction of the centre, is as the force and as the square of the time; the time is given by the area of the sector described; hence the force is as the deviation from the tangent divided by the square of the area. Newton expresses it by saying that the force is inversely as a solid, the product of two line squares divided by a line being of the dimension of the cube of a length.

In proposition XI the computation is made for an elliptical orbit in which the centripetal force is directed to the focus. It is a computation

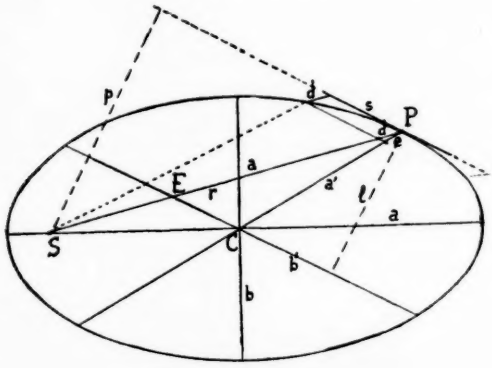

FIGURE 2

indeed, where by making use of the geometrical properties of the ellipse and the lengths of lines therein, a simple result comes out. Drawing in Figure 2 from the planet's place P the conjugate diameters $2a'$ and $2b'$, drawing the perpendiculars l and p, remembering that $PE = a$, denoting the planet's motion by s and its deviation from the tangent toward the focus, S, by d, we have to make use of the following properties and equations: (1) $l \times b' = a \times b$; (2) the deviation d toward S stands to the deviation c toward the centre C as $a : a'$; (3) the product $2a' \times e$ stands to the square of s as the square of a' stands to the square of b'; (4) the area described is $\frac{1}{2} s \times p$ where $p : r = l : a$. Then the expression of the force is given by

$$\frac{d}{s^2p^2} = (\text{by 4, above})\ \frac{da^2}{s^2l^2r^2} = (\text{by 2})\ \frac{ca^3}{a's^2l^2r^2} = (\text{by 3})$$

$$\frac{a^3}{2b'^2l^2r^2} = (\text{by 1})\ \frac{a^3}{2a^2b^2r^2} = \frac{1}{2Lr^2}$$

where L stands for the latus rectum b^2/a. Thus the law of inverse squares follows from the elliptic motion about the focus as the centre of the areas proportional to the times.

In modern treatment usually the elements of the elliptic motion, semi-axes and period of revolution, are at once introduced; the area described in time t, $\frac{1}{2} sp = \pi abt/T$, and the deviation from the tangent is $\frac{1}{2}gt^2$, if g is the acceleration of the force, hence the expression for the force used above is

$$gT^2/8\pi^2a^2b^2,$$

and for g we find

$$(4\pi^2a^3/T^2) \times (1/r^2),$$

where in the factor before $1/r^2$ is expressed (because of Kepler's third law) the independence of the force from whatever planet we take.

Newton restricts himself here to demonstrating the geometric proportionalities; but he extends his work by deriving important general propositions on curvilinear motion and treating a number of other cases. In the case of an elliptic motion about the centre of the ellipse, the treatment makes use partly of the same relations as used above: (1) and (3) hold in the same way, for (2) we have the deviation e replacing d, and for (4) we now have the area $\frac{1}{2} l \times s$. Then the expressions for the force is

$$\frac{e}{s^2l^2} = (\text{by 3})\ \frac{a'}{2b'^2l^2} = (\text{by 1})\ \frac{a'}{2a^2b^2}.$$

So the centripetal force towards the centre of the ellipse is directly as the distance of the body from the centre.

Since the ellipse is only one of the conic sections, Newton of course treats also the other ones. For the force directed to the centre only a

few additional words are needed to show that the same demonstration holds, and that in the parabola there will be a constant force, representing Galileo's case, and in the hyperbola there is a repulsive force varying as the distance. For the force directed to the focus the problem could with the same brevity be reduced to that of the ellipse; ". . . but because of the dignity of the problem and its use in what follows, I shall confirm the other cases by particular demonstrations." Thus he gives a separate demonstration that for a hyperbolic and for a parabolic motion the force directed toward the focus is as the inverse square of the distance. Since the importance of the conic sections now comes forward so strongly Newton treats them at length in a wealth of demonstrations and constructions.

IV

These mathematical propositions find their application in the third book, where first the "rules for reasoning in philosophy" are given. We find there as the first ones: "(1) We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances; (2) therefore to the same natural effects we must, as far as possible, assign the same causes." Then follow the "phenomena," which restrict themselves to the Kepler's third law connecting periods and distances for the Jupiter satellites, for the Saturnian satellites, and for the planets, and to Kepler's second law of areas holding for the moon. Then from these phenomena it is deduced that they all are drawn to their central bodies by forces that are inversely as the squares of the distances. For the moon a numerical computation is made: its distance 60 semi-diameters of the earth, its time of revolution $27^d\ 7^h\ 43^m$, the circumference of the earth 123,249,600 Paris feet; then deprived of its velocity it would descend towards the earth in one minute 15-1/12 Paris feet. If, then, this force increases, in approaching down to the surface of the earth 60×60 times, such a falling body at the surface of the earth ought to descend in one second the same 15-1/12 Paris feet. Huygens had deduced from the length of a pendulum oscillating seconds, which he found to be 3 Paris feet 8½ lines, the space described by a falling body (through multiplication by $\frac{1}{2}\pi^2$) to be 15 feet 1 inch 1-7/9 line. "And therefore the force by which the moon is retained in its orbit becomes, at the very surface of the earth, equal to the force of gravity which we observe in heavy bodies there. And therefore (by Rule 1 and 2) the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity. . ." It is well known that when first conceiving this idea in 1666, Newton made the computation with too small a value of the circumference of the earth, and only in later years could repeat it with the right values. He now strengthens

his argument by presenting it in a "Scholium" in another way: supposing the earth to have more moons just as Jupiter and Saturn have, connected by Kepler's third law, and one to move near the surface, grazing the mountain tops, then it would exhibit a centripetal force obliging it to descend just the same amount in a second as a free falling body at the same mountain top; hence these forces must be identical. "Because otherwise this little moon at the top of a mountain must either be without gravity, or fall twice as swiftly as heavy bodies are wont to do." And then the conclusion is made: "The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of that centripetal force which retains the moon in its orbit will extend itself to all the planets."

Now the consequences are drawn. The moons of Jupiter gravitate towards Jupiter, the planets towards the sun. A power of gravity is tending to all the planets; Jupiter also gravitates towards its satellites, the earth towards the moon, all the planets gravitate towards one another. Since all the heavy bodies descend to the earth from equal heights in equal times (Newton tested it by pendulum experiments) their weights are proportional to their quantities of matter. "Forces which equally accelerate unequal bodies, must be as those bodies; that is to say, the weights of the planets towards the sun must be as their quantities of matter." Newton takes into consideration the possibility that it could be otherwise: "if some of these bodies [Jupiter's satellites] were more strongly attracted to the sun in proportion to their quantity of matter than others, the motions of the satellites would be disturbed by that inequality of attraction." The weights of bodies towards different planets, hence the quantities of matter in the several planets, computed from the distances and periodic times of revolving bodies, are found to be 1 for the sun, 1/1067 for Jupiter, 1/3021 for Saturn, 1/169282 for the earth (from a solar parallax of 10".5). "The force of gravity towards any whole planet arises from, and is compounded of, the forces of gravity towards all its parts . . . If it is objected that . . . all bodies with us must gravitate one towards another, whereas no such gravitation anywhere appears, I answer that . . . the gravitation towards them must be far less than to fall under the observation of the senses." "The force of gravity towards the several equal parts of any body is inversely as the square of the distance from the particles."

V

The resulting principle that gravity is a universal force, exerted as an attraction from every body and every particle of a body, makes a reconsideration of some of the arguments necessary. The mathematical treatment was already given in the further sections of Book I.

Newton here always speaks of attractions. In the Scholium at the close of Section XI we read: "I here use the word attraction in general for any endeavor whatever, made by bodies to approach each other, whether that endeavor arise from the action of the bodies themselves, as tending to each other or agitating each other by spirits emitted; or whether it arises from the action of the ether or of the air, or of any medium whatever, whether corporeal or incorporeal . . ." After the mathematical investigation of the forces and the physical comparison with the phenomena of Nature "we argue more safely concerning the physical species, causes and properties of the forces. Let us see, then, with what forces spherical bodies . . . must act upon one another; and what kind of motions will follow from them."

Thus the attraction of extensive bodies is treated in Section XII. In the former demonstrations the centripetal force was directed to a point, and it was tacitly assumed that the centre of the solar or the planet's globe should be that point. But in reality the forces are attractions proceeding from the separate particles, and then we have to find the sum of them. It is known that Newton struggled a long time with this problem, and considered his work achieved only when he had solved it. This solution consists in the demonstration that a spherical surface layer does not exert any attraction upon a corpuscle inside, and attracts a corpuscle outside with a force inversely proportional to the square of its distance from the centre. The same holds for complete spheres if every spherical layer within is homogeneous. So it was legitimate to simplify the attraction of a celestial body as if it were condensed in its centre. A large number of demonstrations for the attraction of spherical as well as non-spherical bodies under different laws of attraction is added.

A second point is the mutual attraction of bodies. "I have hitherto been treating of the attraction of bodies towards an immovable centre," thus Section XI of the first Book begins, "though very probably there is no such thing existent in nature. For attractions are made towards bodies, and the actions of the bodies attracted and attracting are always reciprocal and equal . . . both revolve about a common centre of gravity." By comparison with the fictitious case of a body P revolving about a fixed body S at their total distance he shows that the two bodies describe concentric similar ellipses about their common centre of gravity, with areas proportional to the times, and in a periodic time $\sqrt{S}/\sqrt{(S+P)}$ relative to the case of comparison. In modern times, after the discovery of double stars, the latter result is more commonly expressed by saying that the periodic time is the same as if in the comparison case the sum total of the two masses were collected in the attracting centre.

Now the sun cannot be the immovable centre of the world any more. It has to abdicate as such in favor of the common centre of gravity of

the entire system; this centre chiefly depends on Jupiter, and in a smaller degree on Saturn, and is situated near the surface of the sun; the sun itself is moving about this point in a somewhat irregular way.

More important, fundamentally, is the mutual attraction of the planets.

VI

Newton, by proceeding from Kepler's empirical laws, derived the universal law of mutual attraction, dominating the motions of the celestial bodies. Thus Kepler's laws, from empirical results based upon observation, hence partaking in the uncertainty unavoidable in any empirical rule, were elevated to the rank of absolute truths, necessary consequences of a fundamental law, rigidly holding, without any uncertainty. But at the same time this fundamental law of universal attraction asserted that Kepler's laws could not be strictly correct, that they were only approximations holding for the case that only two bodies existed, the sun and the planet; or, more correctly, that all the planets had masses infinitely small.

Though this apparent contradiction—that the foundation of the structure was destroyed by the structure itself—could not seriously disturb Newton in his derivation, we perceive it in the way in which in his discussion he first has to minimize the deviation. "But the actions of the planets one upon another are so very small, that they may be neglected" (in Prop. 13 of the third Book). Even in the case of the moon, where in Prop. 3 he enounces that the force by which it is retained in its orbit is inversely as the square of its distance from the earth, he says that this is evident "from the very slow motion of the moon's apogee; which, in every single revolution amounting but to 3° 3′ forwards, may be neglected."

He does not restrict himself to assertions only. In the first Book, after having derived the centripetal force for a fixed ellipse he treats the case of a body moving in a curve that itself revolves about the centre of force. Because, compared with the fixed ellipse, the body has to perform an additional transverse motion, it has, in order to arrive at the same distance, to perform an additional deviation towards the centre, requiring an additional force. Newton demonstrates in Prop. 44 that this additional force varies as the inverse cube of the distance. For the case of orbits approaching very near to circles he is able to deduce a number of numerical corollaries. If the centripetal force varies as the $(n-3)$th power of the distance, the descent of the body from the upper apse to the lower apse will take place over an angle $180°/\sqrt{n}$; hence for $n=4$ over 90° (elliptic motion about the centre), for $n=1$ over 180° (elliptic motion about the focus), for $n=0$ there is no apse, and the motion takes place, as was demonstrated in Prop. 9 already, in a logarithmic spiral. If the centripetal force is a composite function, consisting of an inverse square power diminished by a

force direct as the distance, $(1/r^2 - cr)$, the body will return to the same apse after $360° \sqrt{(1-c)/(1-4c)}$; for $c = 1/357.45$ we find that in each revolution the apse will go forward 1° 31′ 28″. "The apse of the moon is about twice as swift." Here Clairaut's later result is forestalled that simple theory gives only half the motion of the moon's apogee.

The perturbations of the moon due to the solar attraction of course offer the first main problems. Newton has treated them in his first Book as a number of corollaries to Proposition 66, dealing with the problem of three bodies in a general way; and then again in his third Book, where especially the retrogression of the nodes is derived. The perturbations of the planets are so much smaller that they hardly are mentioned, or only as insignificant. "It is true," Newton says, "that the action of Jupiter on Saturn is not to be neglected . . . the gravity of Saturn towards Jupiter will be to the gravity of Saturn towards the sun as . . . 1 to about 211. And hence arises a perturbation of the orbit of Saturn in every conjunction of this planet with Jupiter, so sensible, that astronomers are puzzled with it. As the planet is differently situated in these conjunctions, its eccentricity is sometimes augmented, sometimes diminished; its aphelion is sometimes carried forwards, sometimes backwards, and its mean motion is by turns accelerated and retarded; yet the whole error in its motion about the sun . . . may be almost avoided (except in the mean motion) by placing the lower focus of its orbit in the common centre of gravity of Jupiter and the sun . . . and therefore that error, when it is greatest, scarcely exceeds two minutes yearly . . ." The perturbation of Jupiter by Saturn is much less. "The perturbations of the other orbits are yet far less, except that the orbit of the earth is sensibly disturbed by the moon," since it revolves monthly about the common centre of gravity. How the insignificance of the perturbations occupies his mind, compared with the effects of the solar attraction, appears where again in the next proposition (14) he asserts: "The aphelions and nodes of the orbits of the planets are fixed. . . It is true that some inequalities may arise from the mutual actions of the planets and comets in their revolutions; but these will be so small, that they may be here passed by." And reversing the usual argument he concludes: "the fixed stars are immovable, seeing they keep the same position to the aphelions and nodes of the planets." And concerning the planets near the sun he says in the Scholium added: "their aphelions and nodes must be fixed, except so far as they are disturbed by the actions of Jupiter and Saturn and other higher bodies." He derives that their aphelions move forward a little as the 3/2th power of their distances from the sun; so that, if for Mars the aphelion is carried forwards 33′ 20″ in a century, this displacement for the earth, for Venus and for Mercury will be

17′ 40″, 10′ 53″, and 4′ 16″. "But these motions are so inconsiderable, that we have neglected them in this Proposition."

This is all that Newton had to say on the perturbations of the planets. His was the great task of establishing the force of universal gravitation as the cause of the laws of planetary motion, thereby at the same time explaining the orbits of the comets—to which a large remaining part of Book III is devoted—the precession of the equinoxes, the tides of the oceans, and, in first indications, the irregularities of the moon. The perturbations of the planetary motions he had to leave to his successors.

VII

When Newton's *Principia* was published in 1687 the vortex theory of Descartes was universally acepted as the explanation of the planetary motions. Compared with old Aristotle who still in the first half of the 17th century dominated the academic chairs, the philosophy of Descartes was a considerable advance. Through the common experience of phenomena of floating objects carried by a stream and of dead leaves rushed by the wind, the idea that the planets were carried along by a light space-filling fluid circulating about the sun—and about the earth and the planets too—looked highly plausible. Huygens in a treatise "On the cause of gravity" had tried to explain gravity by applying his results on centrifugal force upon this revolving world aether. So Newton had to devote considerable time and space to the study of the motion of fluids and of bodies contained in them, which occupies his second Book. This Book contains far more than was necessary for a criticism of the prevailing ideas; but it makes the criticism of the vortex theory the more stringent. What is expressed in the concluding General Scholium of the entire work in the mild sentence: "The hypothesis of vortices is pressed with many difficulties," is demonstrated strictly and in detail in the end of the second Book: "Hence it is manifest that the planets are not carried round in corporeal vortices." The criticism was crushing, embodying the progress, in half a century, from vague philosophical talk to the clear mathematical and scientific demonstration.

So it might be expected that Newton's theory immediately, as by storm, would have conquered the minds of the fellow-scientists and replaced the old ideas. Nothing of the sort happened. It is true that in England he soon found admirers and adherents, who spread his new philosophy by smuggling it in the form of footnotes into a generally used textbook on Cartesian physics. But on the continent during half a century it remained unappreciated. The great physicists there knew his work and admired its mathematical demonstrations but did not see what it meant; its entire spirit was foreign to them. As Huygens wrote in 1691 in a reprint of his former treatise: "I had not thought of this regular decrease of gravity, namely that it is as the inverse square of

the distance; this is a new and highly remarkable property of gravity, of which it is worthwhile to investigate the cause." Here it is evident that for him the cause of the planetary motions was not at all cleared up by Newton. The pressure of some world-filling medium against objects as applied in the vortex theory was an understandable cause of their moving; so Newton's law may show a way to clear up the character of this medium and its pressure, but that is all. And Newton's opinion that all particles, even deep inside the earth, should attract each other, looks quite absurd to Huygens because he thinks he sees clearly "that the cause of such an attraction cannot be explained by any principle of mechanics or by the laws of motion." And further on, speaking of gravity, he says "It would be otherwise if we should consider gravity an inherent property of the corporeal matter. But I do not believe that Newton thinks of that, for such a supposition would lead us too far astray from mathematical and mechanical principles." And Leibniz in a letter to Huygens still more clearly gives expression to their reluctance: "It seems that to him [Newton] gravity is nothing but a certain immaterial and inexplicable power (vertu), whereas you explain it very well through mechanical laws."

The curious thing is that in what here appears as a fundamental difference of philosophical opinion, Newton himself agreed with them. We quoted already some of his sentences about the causes of gravity and revolution. In a letter to Bentley he wrote: "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else . . . is to me so great an absurdity . . ." Notwithstanding the often quoted proud statement in his concluding chapter: "Hypotheses non fingo," he was not different from his contemporaries in wanting and fancying explanations of the phenomena through understandable causes. In private letters to his friend, Robert Boyle, he tries to explain the cause of gravity by means of a fine elastic "aetherial substance" filling up all space between the minute particles of the bodies. But also in the *Principia* itself this point of view is exhibited clearly enough. "Hitherto we have explained the phenomena . . . by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and planets. . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. . . And to us it is enough that gravity does really exist, and acts according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies."

Thus the merit and the greatness of Newton's work seems to be his resignation, his restriction to computation of effects by mathematical formulas. Certainly it was this that secured its victory. People could work with his theory. When he himself, and, following his traces, first English and then French theorists succeeded in computing and predicting ever more phenomena by means of the law of gravitation, when theory went from triumph to triumph, the old vortices, unable to produce anything of the kind, fell flat, disappeared and were forgotten. Since the middle of the 18th century the truth of Newton's theory was universally recognized. Truth, a modern writer says, is what you can work with. And now gradually there came another fundamental scientific creed. Gravitation did not have or want a cause; it was itself a cause. Though Newton had vehemently rejected the idea that he should believe in "action at a distance," scientists in the next century ever more considered forces acting at a distance a sufficient explanation of motions, and proclaimed them the real, essential, and sufficient causes of the observed phenomena, introduced wherever, as in electrical or magnetic phenomena, attraction or repulsion was observed.

In reality, when we say that gravity is the cause of bodies falling, we explain nothing. Gravity is a name, a word, that does not contain more than do the phenomena themselves; in calling them by this name we express their general character. By expressing the general characte. by a law, a mathematical formula, we are able to predict, to compute the motions of all falling or thrown bodies. The law embodies them all; it is the abstract concept our mind constructed out of the phenomena. Newton's work consisted in that he explained the celestial motions by extending earthly gravity over all bodies of the universe and finding its dependence on distance. So it is not, as it might seem at first sight, an attempt to explain an unknown by reducing it to another unknown. The "explanation" consists in that Newton's gravitation embodies a far wider field of phenomena into one common rule. Thus our image of the world is simplified; an endless multitude of the most diverse experiences in heaven and on earth, in the past and the present (and the future), is condensed in a single law, in a formula from which they all can be computed.

It is needless to look for further causes or to ask what gravitation is. Gravitation as a special something pulling at the bodies and steering them through space has no more separate existence than Snell's law of refraction as a somewhere given command to the light rays how they have to run through the different media. If they are called "causes" of which the phenomena are the "effects," it must be borne in mind that to modern science cause means the short summary or compendium, effect means the diverse multitude of phenomena. In this sense, then, Newton's gravitation is rightly called the cause of the heavenly

motions. That Newton, though occupied by the ideas of his time, restricted himself to the mathematical treatment, means more than that it made his work fruitful and persistent. It means too that, unconsciously, he opened at the same time the way to a new conception of science. What in Newton's work, even to himself, looked like a resignation, was in reality the first hesitating expression of what would become the epistemological foundation of modern science.

Light Absorption in Colored Glass

By H. B. RUMRILL

The refraction of light through transparent media is attended with numerous interesting phenomena. The familiar bending of the rays when passed into a vessel of water, as also the loss of light by absorption either in water or glass—dependent upon the clarity of these substances—are among the results having optical consequences.

In general it may be said that the thicker the glass the greater the loss of light by absorption, accordant with its purity, which accounts for the diminution of light, per unit of surface, with the increased thickness that necessarily accompanies enlargement of area of telescope object glasses, and its bearing on limitations of aperture.

This is one of the reasons why inverting ("celestial") eyepieces of two lens formation, such as the Huyghenian and the Ramsden, are better for astronomical purposes than the ordinary four lens erecting ("terrestrial") magnifiers; although Proctor found solar observation through the latter advantageous. This was partly because the dark glasses for eye protection could be thinner or of somewhat lessened absorption, due to the additional absorption occasioned by the erecting lenses.

To a lesser degree this principle applies in the employment of the Herschel helioscope for solar observation; its use when looking at the Moon requiring no modification of prismatic reflection except with the larger apertures.

However, the remarkable light transmissive properties of the finest optical glass make it practicable to construct compound eyepieces of various kinds, and also to gain the convenience of right-angled prisms for diverting the emergent rays diagonally, with but little loss of light. Hence it is feasible to make use of the so-called "solid" eyepieces (really thick single lenses) and of cemented combinations such as the "monocentric" and "orthoscopic" oculars—with all of which inner reflections are pleasantly absent. (My equipment includes a half-inch erecting eyepiece the lenses of which are so perfectly transparent that it is difficult to perceive loss of light.)

With such an intensely bright object as the Sun, the reduction of

light afforded by the direct interposition of colored or neutral tints of glass, including the screens known as "London Smoke," is obligatory for eye protection and satisfactory observation with very small telescopes. For larger instruments the admirable control of light value in the Merz form of polarizing helioscope renders any such additions unnecessary and, in fact, undesirable, the uncolored solar image setting a standard with which other methods of dimming the light may be compared.

In a fairly large collection of screen glasses gathered from many sources, I have a number of varieties of "London Smoke," including some that have a predominance of green, blue, yellow, or grayish violet. Where these are rather thin or lightly tinted, combinations are possible with good effect, for instance green with red (if the latter is not too pronounced), and green with cobalt blue or yellow. This is mentioned by Webb in "Celestial Objects," who also alludes to the German preference for deep yellow screens. By chance I came across two such eyepiece caps as part of the equipment of an old Plössl "dialyte," which I found serviceable on my telescope.

We read of the former alternative employment of a "trough" of inky water within the eyepiece, and of the combination of a "drum" of alum water (said not to transmit heat) with dark spectacles; still it seems hardly credible that either method could have been an eligible substitute for glass filters having optically polished surfaces, that is, when used with the wedge prism helioscope. The old-fashioned expedient of smoked glass has some advantages, though equality of absorption is not usually one of its attributes.

Regarding these artifices, in the 1821 edition of Brewster's "Ferguson's Astronomy," the chapter on New Discoveries respecting the Sun begins with a reference to the use of *black* glass with the telescope, "to intercept a portion of the solar rays," the presumption being a screen of low absorption. Brewster goes on to say that

> It is a remarkable circumstance, that Fabricius [1564-1617] was acquainted with no method of intercepting a portion of the solar rays, in order to save the eye. He observed the Sun when he was in the horizon, and when his brilliancy was impaired by thin clouds and floating vapours; and he advises those who repeat his observations to receive at first a small portion of the Sun, and gradually to accustom the eye to a greater portion, till it is able to bear the full blaze of its light. When the altitude of the Sun became considerable, Fabricius was compelled to abandon his observations; and he informs us, that his eye was so much affected by the impression of the solar light, that, during the two following days, he could not see objects with the same distinctness as formerly.

Further on in this interesting chapter it is stated that

> Dr. Herschel has invented a very ingenious contrivance for moderating the heat and light of the Sun, when it is examined by means of powerful telescopes. He abandoned the common method of using dark-coloured glasses, and had recourse to fluids. For this purpose, he employed a small square trough, having, in two of its opposite sides, well polished plates of glass. A small handle in one side of the trough, and a spout in the other, were made, for the purpose of pouring out any portion of the liquid when the rest was to be diluted. The trough was then placed in an excavation in the eyepiece of the telescope, so that the rays of the Sun might pass through the fluid before they reached the eye of the observer. By colouring the fluid, the light may be softened at pleasure, and the heat is completely removed by the water. Dr. Herschel found that ink, diluted with water, and filtered through paper, gave a distinct image of the Sun, as white as snow. By this mixture, he could observe the Sun in the meridian, without the smallest injury to his eye, or to the glasses, even when he used a mirror nine inches in diameter, and when the eyepieces were open, as in night observations.

The idea of utilizing a fluid in solar observation was long since embodied by Zeiss in the "Sun Prism after Colzi"—a combination of concave lens and mirrors with glass and fluid prisms. The catalogue explains that "by a suitable choice of liquid the rays may be reduced to a convenient intensity for viewing the Sun's image and the intensity varied within wide limits by rotating the glass prism body with respect to the glass mirror." Another form of fluid helioscope was also brought out many years ago by Hilger of London.

Some of the early seventeenth century observers of the Sun made use of colored glass to lessen its brilliancy, or else, as in the case of Fabricius, confined their observations to the times when fog or the atmospheric thickness along the horizon absorbed enough of the light. But at low altitudes the coarsest details only can be thus seen; yet with a very thin fog a lightly tinted screen glass would permit the sighting of features within the capacity of the magnifying powers then available. I have looked at the horizon Sun under these conditions with a small telescope minus screen, but caution is necessary as a slight increase of altitude quickly lets too much light through. Needless to say, such observations do not allow the application of sufficient power for the detection of the finer details of maculae and faculae; this again, in the case of fog, depending upon its density,—and it is seldom of just the right consistency for this purpose.

In solar observation, therefore, our astronomical ancestors had to put up with a good many discouragements, including in addition to these

difficulties narrow field of view and faulty mountings (or none at all) for their inferior instruments, their "best bet" being the projection method—practically as now used. It seems hardly likely that they could have even dreamed of the superb products of modern skill.

In the matter of filter colors, the central regions of the spectrum are superior for visual work to the extremes; red particularly being objectionable because of its partiality for the heat rays so injurious to the eyes. I have a Zeiss compensating wedge of neutral tint with clear glass—the color verging on yellow slightly tinged with green—that has proven satisfactory in the study of photospheric phenomena.

The human eye is so constituted that it cannot tolerate superabundant light, and astronomical history records the loss of sight following attempts to explore the solar surface telescopically without adequate eye protection, as well as the unfortunate results of experimenting with naked-eye vision of the Sun—a few seconds of such exposure sufficing to cause blindness. While the mechanism of the eye, with its involuntary pupillary control, provides a wide range of "accommodation" it has limitations that must be augmented artificially in order to correctly visualize brilliantly illuminated objects. The eye is strained by too little as well as too much light, hence the need for broad optical surfaces to enable perception of faint nebulae and star clusters, and for the means of tempering the light of intrinsically bright objects.

The writer recalls quite vividly an old experience of a prolonged observation of the full Moon with a 2½-inch telescope, that resulted in the retention for several hours of a retinal impression of a purple Moon—a complementary color. According to Chevreul, "the retina affected by the yellow has a tendency to see violet." The disagreeable experience was never repeated.

With small telescopes a light blue screen reduces bright moonlight to a bearable quantity; as also a thin glass of neutral tint. For this purpose apertures of great light-gathering capacity require deeper hues of eye protecting glass; or, better yet, the wedge prism alone of the Herschel helioscope. This invaluable device is also probably the best way of looking at the planet Venus in a dark sky, as it *rejects* surplus light, but it may be dispensed with in the brightening twilight of early morning, or in full daylight, which naturally absorb the glare.

In Lockyer's "Stargazing" (1878) it is stated that ". . . the minute 'granules' of Dawes are best seen with a blue glass, but for observing the delicately-tinted veils in the umbrae of the spots a glass of neutral tint should be used," and "to observe the fainter satellites of the brighter planets, or, indeed, faint objects generally, near very bright ones, the bright object may be screened by a metallic bar, or red or blue glass placed in the common focus."

In screen glasses a desideratum is the homogeneous admixture of the color-producing metals or metallic oxides with the materials that are

fused together in making optical glass. I recall some thin red glasses shown me by the late Mr. McDowell of the Brashear Company, who remarked on the difficulty of preventing a certain streakiness (a kind of striae) causing lack of uniformity in texture. I have not noticed this peculiarity with other screen colors and have specimens of red that lack anything of the sort.

Related to this it might be added that the selection of suitable combinations, including that of light neutral tint with yellow, requires careful discrimination to obtain proper blending. This is on a parity with the union of the metallic sources of color into what might be called alloys for the production of truly neutral tints that may reduce light intensity without hiding fine details—in other words retaining essential transparency.

Regarding color blending, I have noticed the good effect of mounting "London Smoke" screens in front of the objectives of a Dollond inverting binocular. These are of somewhat different absorption as well as color, and they coalesce well when thus used stereoscopically. As the surfaces are polished accurately flat, there is no distortion.

For the reason that visual object glasses of the usual type are corrected to the sodium wave length of the spectrum, a filter of that color serves to adapt the refracting telescope to photography, by favoring the actinic rays. So arranged, I have found it easy to obtain good photographs of the Moon, either at the initial focus or through low power eyepieces, with the screen close to the plate.

Many years ago some of the colors of the spectrum were successfully imitated in glass produced at Jena. These colors—red, yellow, green, blue—are exquisite, my acquaintance with them coming through the late Mr. Petitdidier, Chicago optician, in 1911. Depth of color may be varied by the thickness of the glass, which appears to be an effectual way of securing desirable scaling of the properties of absorption, especially with the denser colors.

Recently I have been using two of the Willson greenish yellow filters in studying sunspots, placing them between the eyepiece and the helioscope prism. Their absorption varies slightly, the darker of the two excluding enough of the residual light reflected by the prism to permit clear vision of the smallest spots when our own atmospheric conditions offer no interference. The faculae are better seen, however, with a neutral tinted glass, the contrast with the perfect white of the photosphere being rather more pronounced than with filters wherein yellow predominates. The observation of faculae detail often requires very careful balancing of light values through the medium of screen coloration; the well graded screens in my Dawes solar eyepiece giving excellent results.

In pursuing this subject I made three discs on the same principle, each with five cells around the circumference, and fitting interchange-

ably over several eyepieces. In these are inserted half-inch screens, classified as neutral tints, faint colors, and Zeiss spectrum colors. In addition to their use singly, experimental combinations are readily arranged and it is interesting to note the variety of effects possible, and their influence on definition.

Another result I have noticed in using the Willson filters is their relation to magnification. Their consistence is so limpid that spot details seem to be as easily seen with low powers—say 60 to 80—as with higher magnifications used with certain other screens. I do not think I am mistaken in this estimate of the Willson glasses, even though allowance must be made for the atmospheric variations that affect or modify all optical combinations.

The sources of color employed in the production of screen glasses include sodium for yellow, selenium or gold for red, oxide of chromium for green, the oxides of copper or cobalt for blue, and oxide of manganese for violet. I understand that the green of bottle glass is due to the presence of iron (ferrous silicate) derived from impure sand or limestone, and have known of the use of pure gold in the making of the red tops of glass pens.

In addition to the foregoing methods of reducing the intensity of sunlight the occasional use of colored lenses is referred to here and there in works on observational astronomy, and while I have never seen anything of the kind I should imagine that it would be practicable to make compound eyepieces with lenses of colored glass, of formulae giving uniform light transmission over the entire field. It is stated by Thornthwaite that colored Barlow lenses have been effectively used in solar observation, but in all probability their restriction to but one class of work has prevented any wide adoption of the idea, as in the case of silvering Barlow lenses and object glasses.

Incidentally, it may be noted that a screen glass of suitable absorption brings out the beauty of cloud formations and of snow scenery. Snow blindness has been prevented by the Eskimo by means of wooden goggles having narrow slits cut lengthwise, effecting elimination of excess light. (Tyrrell's "Across the Sub-Arctics of Canada.") I recall using a pair of dark violet spectacles when looking at the molten iron in a furnace in the Pittsburgh district, and the extraordinary brilliancy of the "pour" from a Bessemer converter, seen from a safe distance in daylight.

Among the older authorities consulted in connection with these studies are Chevreul on Color, Field's Chromatography, Rood's Modern Chromatics, Bradley's Elementary Color, Hovestadt's treatise on Jena Glass, and von Bezold's Theory of Color. A recent (1938) source of information is the Circular of the National Bureau of Standards entitled "Spectral-transmissive Properties and Use of Colored

Eye-protective Glasses," by Coblentz and Stair, covering many modern developments.

The applicability of other forms of experimentation in the technology of color mixture to the problems of light modification with optical instruments renders it helpful to learn the outcome of practical work in other directions, because originating in the analysis of spectral decomposition of light. Whilst there is dissimilarity of purpose as between science and art, there follow general deductions relevant to color combinations in their relationship to telescopic definition in dealing with glass used as screens or filters.

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A Review of Theories on the Origin of the Earth*

By WILLEM J. LUYTEN

Almost from their very beginning theories on the origin of the solar system can be classified into two kinds: the internal, and the external type, either a gradual, almost inevitable evolution or a sudden cataclysmic stroke. Nearly two centuries ago Kant visualized the planets as having been formed from the gradual evolution and development of a chaotic mass of matter: a primeval cloud of meteorites or a nebula, but a little while later the French naturalist Buffon saw it all as the result of a collision between the sun and another body which he called a comet but which he endowed with such properties that now we should have to call it a star.

What we may call the two great theories in the field showed again the same division: Laplace in his celebrated Nebular Hypothesis followed Kant, but Chamberlin and Moulton, and later Jeans and Jeffreys, were on the side of Buffon.

Briefly, Laplace's theory amounts to this: originally all matter in the solar system was contained in one huge spherical nebula, perhaps 10,000 times the diameter of the present sun and endowed with a slow rotation. With the passage of time gravitation would do its work, the nebula would shrink and therefore increase its speed of rotation and hence become flattened. Eventually this would reach a stage where gravitational attraction would be unable to supply the necessary centripetal force, instability would set in and the nebula would throw off a ring, which would, in turn condense into a planet. The process would repeat itself time and again and thus we see emerge a system possessing all the principal charactertistics of our solar system since all the

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planets would move in the same direction in essentially the same orbital plane. Now it is a curious fact that every stage in this Laplacean evolution process is represented by objects we can see in the sky: there are many spherical and elliptical nebulae, there is even a ring nebula, and, of course, there is the planet Saturn with its rings. Yet the laws of matter as we now understand them make it impossible for us to accept the Laplace theory: when the rotating nebula became unstable, it would not throw off rings but matter would diffuse from the equatorial periphery; and a ring, even if formed, would have a little difficulty condensing into a planet—in fact what happened in the case of Saturn is almost certainly the Nebular Hypothesis in reverse. Finally the Laplace process should have left the sun the fastest rotating object in the system whereas actually it is the slowest. The planets whose present mass is only 1/700 of the entire mass of the system contain more than 98 percent of the total angular momentum, and the sun, with nearly all the mass has less than two percent of the angular momentum.

While any one of the objections voiced above appears to be fatal, the most serious and fundamental one seems to be that involving the angular momentum, especially in view of later work by Moulton and Jeans which indicated that perhaps this difficulty is inherent in this type of theory.

A complete reversal of attitude was thus indicated, and Chamberlin and Moulton promptly came with a theory that did just that and which attributed the origin of the planets to the effects on the sun from another star which passed very close to its surface. The beauty of this idea is that now the angular momentum of the solar system has been imposed from without: now the sun is *expected* to be slowly rotating.

In the original theory Chamberlin saw the planets as being formed by a combination of causes: the tidal wave caused by the passing star plus the expansion of the highly compressed solar gases, plus another eruptive force which we see at work in solar prominences. Again the stages assumed in this genesis of the planets are those which we observe in the sky. Then, once the outward flow of gases from the sun's surface had begun, this stream would be pulled sideways by the rapidly moving star; it would cool rapidly as well and countless little condensations would form in it: these are the "planetesimals" from which the theory derives its name. Eventually these little particles would coalesce into larger masses and form the present planets.

In Jeans' later version of the same theory the tidal wave—to put it very crudely—raised by the passing star would suffer fragmentation first and it is these fragments which, cooling rapidly, would condense into the planets. When many difficulties arose with this picture Jeffreys changed the theory still further into one involving an actual collision between the sun and another star. A filament of hot gases would be torn out of the sun as the two objects separated, and this filament

would break into fragments which in turn would condense and form the planets.

And so, the cycle was complete again: first came Buffon with his collision with a comet, then Laplace with his nebular hypothesis, followed by the planetesimal, then the tidal theory, and finally again we are back with the collision theory.

Now the arguments against the last three theories are roughly the same and in the main they come down to these:

Firstly, the fact that close approaches between two stars are very rare and actual collisions are excessively so gives these theories a certain aura of improbability. But this is not taken too seriously, except perhaps by some proponents of the theory who go to such lengths in minimizing the objection that it looks like another case of "methink the lady doth protest too much."

Secondly, if a passing star is to raise huge tides on the sun it must (a) be of large mass, and (b) come very close. Now we know many varieties of stars of large mass, but they are so large in size that they could not come close, and a star that satisfies both requirements still has to be invented.

Thirdly, some fifteen years ago we thought that the sun was composed of about one-third hydrogen by mass and pulling planets out of the solar atmosphere seemed only a minor operation then. But by now the hydrogen content appears to have been raised to 90, or 92, or maybe even 95 or 99 percent, and that would mean, since the planets are composed largely of metals and silica, that the original planetary matrix may well have had more mass than the sun has now, and the whole process becomes not merely a major operation, but more resembles the partition of Poland.

Fourthly, there is the angular momentum difficulty. Simple calculations indicate that the planets have much more angular momentum than any passing star could reasonably have imparted to them. And so we are back doing business at the same old stand: in the Laplace theory it was the sun that had too little angular momentum, in the encounter theory the planets are found to have too much. There have been some further attempts to get around this difficulty, one ended up with a game of cosmic billiards where a binary star of which the sun was one component, suffered a collision with a passing star, but this and several more theories produced in rapid succession by the same authors can hardly be taken seriously: they are little more than exercises in college algebra.

But that does not mean that the field is not active now. Some years ago Nolke tried a possible origin of the solar sytem out of a spiral nebula where, of course, there are all kinds of angular momentum available, but just because of this, the theory that comes out of it is bound to be somewhat vague, and involving many *ad hoc* assumptions,

such that it is difficult to prove or to disprove it. Schmidt in Russia has proposed a theory which sees the origin of the planets as due to the sun's travelling through clouds of meteoric matter with subsequent capture of part of the cloud and condensation of the captured particles into planets. Magnetic forces, the origin of binary stars, and other ideas have likewise featured in recent theories, but unfortunately, there is not time to give even a brief description of all of these, and I shall try only to give an outline of the salient points of one of the recent theories, *viz.*, that proposed by Berlage in Holland. It might be added perhaps that his first ideas on this go back to 1930 and as such he is the forerunner of Weiszacker; the latest phase of this theory which is as yet unpublished but which I am permitted to mention here was worked out largely while the author was in a Japanese concentration camp in the East Indies during the war.

The sun is pictured as surrounded by a very extensive gaseous envelope which, because of rotation took on the shape of a flat disk, and which subsequently began to resemble a tore. Obviously the temperature and the mean molecular weight in this gas would have to decrease from the center outward and in order to fix ideas Berlage makes the assumption that they both decrease in the same way such that T/μ remains roughly constant. He then investigates the possibilities of turbulence and finds that this will be restricted to a region outside Neptune's orbit at least in the beginning. Further he finds that condensations should occur at distances from the sun roughly corresponding to those of the present planets and indicating an outward decrease in the mean density. The present ideas on the very high hydrogen and helium content of the sun are a difficulty here too but the obvious answer to it is the almost quantitative escape of these lighter gases from the disk-shaped nebula at an early stage, leaving an average molecular weight of the gases of around 60 at the distance of the earth, and decreasing to about 12 at Neptune's distance. To go to the formation of satellites is but the next step and here Berlage finds that the conditions for the production of satellites in pairs—2 or 4—are much more favorable than for singletons, a fact borne out by observation. In a sense Berlage's theory combines some of the features of Descartes' vortex theory, Kant's disk, Laplace's Nebula, and Chamberlin and Moulton's planetesimals, but the origin of the disk is still somewhat vague and obscure.

Perhaps we may summarize the present situation by looking at the principal characteristics of the solar system and try to draw some inferences from those as to what kind of a theory we are looking for. Some years ago, and in order to stress his choice of "catastrophism" as against "uniformitarianism" Russell stressed the uniqueness of the sun and the solar system, it being by far the most complex system known. Now it is perfectly true, of course, that the most complex

multiple star system we know has only six components (Castor or Alpha Crucis), whereas the sun has nine planets, nearly thirty satellites and innumerable minor planets and comets. But if we viewed the sun from the same distance as even the nearest of all stars it would be utterly impossible for us, with our present means to detect the presence of even a single planet, and the sun would certainly appear to us as a single star. Against that, the vast majority of the nearer stars in space which we can observe in some detail, prove to be double or triple, and if we take that into account it is seen that we should reverse Russell's conclusion: if the sun is to be considered near-unique it must be because most stellar systems are *more* complicated than ours. My own feeling is that the sun should not be considered in any way unique or singular until we have definite proof for such a view and hence the idea of a unique process for the origin of the planets must be abandoned at present.

If we look at the planetary system carefully we find that it presents a number of striking characteristics which suggest its having been built up from *within,* by a process of gradual development. The near circularity of planetary orbits, not to be compared with the average double star orbit, the near perpendicularity of the sun's rotational axis to the invariable plane, the reproduction in miniature of the planetary system in the satellite systems of Jupiter, Saturn, and Uranus, and the fact that the satellites of these planets move in the planes of their primaries' *equators* rather than of their primaries' *orbits;* all these things suggest to me that we should look for an *internal* type of theory.

Antarctic "Seeing"

By ROBERT S. DIETZ

During a recent trip to Antarctic regions as a civilian scientist aboard a ship of the Western Task Group of Task Force 68 of the U. S. Navy Antarctic Development Project, 1947 (Operation HIGHJUMP), this writer was impressed by the crystal clearness of some of the Antarctic days, the stellar brilliance of some of the nights, and the consequent fitness of these regions for making celestial observations even from sea-level elevation.

Polar regions are noted for many curious optical phenomena, most of which are related to excellent visibility rather than to optical distortion. It is well known that, owing to atmospheric clarity, the mariner and the explorer in high latitudes frequently underestimate distances by as much as 50 miles, and occasionally, by as much as 200 or 300 miles when superior mirages or loomings are involved. Regarding celestial observations, Byrd (1) states that observers at Little America noted as many as 60 meteors per minute. He attributes such

exceptionally high counts to the clarity of the sky, which permitted the sighting of faint trails that would be invisible in other parts of the world.

On clear days, icebergs stand out with such definition that one feels he might reach out and touch them, although they are obviously many miles away. The topmost portion of large bergs can still be seen when the base is so distant that due to curvature of the earth, it is well below the horizon. The sun, which sets with little distortion or change in color, is so brilliant that one cannot look directly at it, until it has almost entirely disappeared below the horizon.

Even more impressive than the clarity of some of the Antarctic days, is the atmospheric transparency of many of the Antarctic nights. A strong beam of light is invisible because of the absence of the Tyndall effect, as there are no solid particles in the air to scatter light. Stars shine brilliantly and can be seen down almost as far as the horizon. Most surprising is the fact that the stars shine with a bright steady light and do not twinkle. All of this adds up to excellent "seeing"—probably unsurpassed anywhere in the world.

This excellent "seeing" in the Antarctic region is largely understandable. As these regions are uninhabited areas of water and ice, there is practically no source for inorganic or organic dust particles, or for pollution by smoke. A permanent high pressure area lies over Antarctica, so that cold, dry, and, consequently, haze-free air flows radially outward from the continent. Strong temperature inversions cause the air to be stable and the airflow to be laminar.

Observations from aboard ship show a low percentage of clear days, but aircraft pilots usually reported good weather over the continent, and their aerial photographs show high atmospheric transparency. Thus is seems that astronomers should take an active interest in investigating portions of the Antarctic continent for possible use in making astronomical observations. Perhaps the ice-free "oases" recently discovered by the Navy near the Antarctic Circle would furnish satisfactory bases. Along with excellent "seeing," the long winter night permits uninterrupted observations and long photographic exposures.

REFERENCE

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U. S. NAVY ELECTRONICS LABORATORY, SAN DIEGO 52, CALIFORNIA.

The Hydrologic Cycle*

By FRANKLIN J. RYDER

> Sing ye to the Lord . . . who covereth the heavens with clouds, which prepareth the rain for the earth. (Ps. 147:8)
>
> The water stood above the mountains. At thy rebuke, O Lord, they fled; at the voice of thy thunder, they hasted away. They go up by the mountains; they go down by the valleys unto the place where thou hast founded for them. Thou hast set a bound that they may not pass over; that they turn not again to cover the earth. (Ps. 104:6-11)

Thus the Psalmist in praising the Creator tersely describes a common cycle of events continually occurring on the earth. This cycle traces the path of a droplet of water from the time it falls as rain on the earth, the course which the water follows over the earth's surface until, by evaporation, it returns again as vapor to the earth's atmosphere. In this process, the sun is the power-furnishing agent.

This hydrologic cycle commences when water vapor in the earth's atmosphere condenses and falls to earth as rain or snow. The precipitation reaching the earth is distributed in several ways (1)†. A part is returned directly to the atmosphere by evaporation. Another part remains on the surface of the earth either as snow, impounded water, or in flowing streams, and eventually flows to the sea or evaporates on the way. The remainder enters the ground as sub-surface water which may be evaporated directly to the air from the soil, assimilated by plants, or drain to surface water by sub-surface flow (See Figure 1). The process by which plants discharge waste water known as transpiration resembles evaporation. Fundamentally this cycle is a continuous exchange of water in its liquid form on the earth's surface to water in the vapor state in the earth's atmosphere.

At this point, the discussion splits into two parts. One section will cover evaporation of water in the above cycle and the effects of solar radiation thereon. The second section will discuss the cycle as a whole and its practical effects.

Evaporation is the process by which water is changed from a liquid to a gas. Such a transformation requires a certain amount of energy known as the latent heat of evaporation (2, 3). If a given mass of water is to evaporate and coincidentally maintain its temperature, energy must be supplied to replace the energy required for evaporation. In nature this energy is supplied through solar radiation (4). From the kinetic standpoint, evaporation occurs when more molecules

*A paper prepared for the October, 1947, meeting of the American Association of Variable Star Observers, Solar Division.

†Numbers refer to Bibliography.

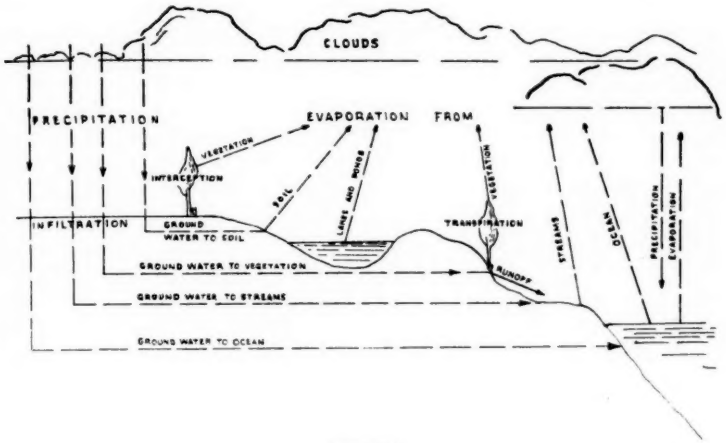

FIGURE 1
SCHEMATIC DIAGRAM OF HYDROLOGIC CYCLE AFTER FIGURE 1, "CLIMATE AND MAN," PAGE 535.

pass through the surface of the liquid to the surrounding gas than pass from the gas into the liquid. Molecules composing the liquid are in constant motion. Those near the surface are restrained in their motion by a strong attraction of the greater number of molecules below them. Nevertheless, a few molecules have sufficient energy to break through the surface and escape as a vapor. The rate at which these molecules leave the surface, or in other words the rate of evaporation, depends on the energy with which these molecules move about in the liquid; that is to say, the motion is dependent on the temperature of the liquid and the vapor pressure of water and the adjacent air. For evaporization to continue, the molecules leaving the surface must be removed from the immediate vicinity. This may be accomplished by diffusion, convection, or wind action.

Mention has already been made of the influence of solar radiation on evaporation process on the earth's surface. As the rate of solar radiation varies, the rate of evaporation varies proportionately. Dr. Abbot and the Smithsonian Institution have done notable work in measuring solar radiation—the "solar constant"—and others have determined empirically annual variations in evaporation rates (5). The point to be emphasized here is that loss of water from the earth by evaporation is primarily dependent on the amount of solar radiation reaching the earth. This is a problem for astronomers.

The hydrologic cycle involves the factors of precipitation, temperature of the earth and air layers, vapor pressure, wind movement, evaporation, and solar radiation. Vary any of these factors, and climate

varies (6, 7). With deficient and poorly distributed rainfall, high temperatures, hot winds, and maximum rates of evaporation, droughts occur. With excessive rainfall, in intensity and volume greater than what soil, vegetation, and land surface can absorb or retain, together with minimum rates of evaporation, runoff occurs; and in individual cases where some combination of intensity and amount of rainfall produces maximum runoff, floods occur. This phase of the hydrologic cycle is the problem of the engineer.

No great amount of imagination is required to visualize the import of the cycle which I have just described. This cycle embraces many sciences such as geology and soil mechanics, ecology and botany, hydrology, solar and terrestrial mechanics. It seems to me that the application of studies on solar radiation to the process of evaporation is a fascinating and useful enterprise.

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Roy Welty Delaplaine

(1886-1948)

By PETER VAN DE KAMP

Roy Welty Delaplaine was born in Waynesboro, Pennsylvania, on January 4, 1886, and died in his home in Swarthmore, Pa., on February 12, 1948. Two weeks earlier he had retired as head of the Physical Education department at Franklin High School, a position he had held since 1926. Thus ended a full life and career, which at night time included an extraordinary amount of astronomical activity of the highest importance to the Sproul Observatory of Swarthmore College.

Mr. Delaplaine graduated from Swarthmore College in 1913 with a major in Mathematics. He did graduate work at the University of Pennsylvania, at Temple University, and at the College Communal, Chatillon-sur-Seine, in France during the first World War. While in the Army he designed the elevating mechanism used on the heavy

Browning machine gun and compiled the first range tables for this gun, which were still in use at the beginning of World War II.

He taught Science for three years at the Wilmington Friends School prior to entering the Philadelphia School System in 1916. He taught at Frankford, Northeast, West Philadelphia, Central, and Franklin High Schools. He also coached football at many of his former schools and at Swarthmore College. In his chosen field, Physical Education, he contributed various articles for journals and newspapers. He was a member of the Swarthmore School Board for ten years, and served as its president during his last six years of service, before the state of his health necessitated his resignation.

ROY WELTY DELAPLAINE
(1886-1948)

Since 1929 Delaplaine assisted with the observations at the Sproul Observatory. Mention should be made of his work with Dr. John S. Hall, which was published in the *Astrophysical Journal*, Volume 88, pages 319-343, 1938, entitled "A Relation Between Color, Spectrum and Absolute Magnitude for G-Type Stars"; also to "A Comparison of the Velocity of Visual and Infra-red Light by an Astronomical Method," *Journal of the Franklin Institute*, Volume 228, pages 411-423, October, 1939. This work won a prize in which Mr. Delaplaine shared. He is the co-author with Dr. Hall of "Photoelectric Observations of Nova Herculis 1934 in the red and infra-red region of the spectrum" published in the *Astronomical Journal*, **45**, pages 197-199, 1936. Particular credit goes to Delaplaine for his successful efforts in securing extensive series of photographs of the nearest two unresolved astrometric binaries, Barnard's star and Lalande 21185 (*Proc. Amer.*

Phil. Soc., Vol. 88, No. 5, pages 372-374, November, 1944). In 1944 he was elected a member of the Society of Sigma Xi.

Delaplaine started his photographic work with the Sproul 24-inch refractor in 1930 and took his last plate on 1947 September 17; during that interval he took 9,600 plates. The total number of plates taken since the telescope was put in operation in 1912 is close to 26,000 (1948 Jan. 1). Roy Delaplaine is responsible therefore for over one-third of the total number. His contribution is actually more impressive if one keeps in mind that he took 8,010 plates, or more than one-half of the 15,119 plates taken since January, 1938. The peak of his contribution was reached during the period 1943-1946, when he carried the night work virtually alone, and averaged better than 1,000 plates a year. It is due solely to his loyalty that we suffered no serious gaps in our series of observations, as several observatories did at that time.

The above statement is simply a quantitative expression of the most basic phase of our photographic work—the observations. Delaplaine has done more for the Observatory. For years he was in charge of visitor's nights; he instructed numerous young observers. His cheerful acceptance of the rigors of the night work, in addition to his full-time activities elsewhere, have been a wonderful example for all of us.

During his later years Delaplaine often waxed poetic while at work in the dome, deriving inspiration from atmospheric interference or from attempts to overcome observational or instrumental difficulties. The resulting ditties he put down in the column "Remarks" of the observing book, characteristically, in capital letters. Several of these seem to be of sufficient general interest to be printed here.

1940 Nov. 8

"NORTH WIND BLOWING
SOUTH MOON GLOWING
IF IT DON'T IMPROVE
TO BED I'M GOING."

1942 Oct. 11 In the morning there had been a minor fire in the dome, presumably caused by self-combustion of oily rags in a closet. That same night—

"LITTLE DROPS OF OIL, A LITTLE 'LECTRIC SPARK,
MAKES THE WOOD BURN BRIGHTLY EVEN IN THE DARK.
WATCHMAN'S KEEN ALERTNESS, FIREMAN'S PROMPT REPLY
SAVES THE DOME FROM BURNING
AND MISTER, I DON'T LIE."

1943 Feb. 2 After a prolonged cloudy spell

"TWINKLE, TWINKLE, LITTLE STAR
'MOST FORGOT WHERE YOU "WAR"
THEN YOU SHOW YOUR SMILING RAYS
AFTER SIXTY CLOUDY DAYS."

1945 Oct. 17

"MUST HAVE DOZED,
SO OVEREXPOSED."

1946 Feb. 4

"CELESTIAL SKY IS NOT AUSPICIOUS
SO I'M GOING HOME TO WASH THE DISHES."

1946 Apr. 13 First plates of the season on Barnard's star.

"ANOTHER YEAR, IN THE SAME OLD BONNET,
SOMETIMES I WISH YOU WERE A "BLOOMIN'" COMET."

We shall miss him very much, and the Sproul Observatory will never be quite the same to those of us who knew him. The energy and devotion with which he passionately dedicated himself to the basic astronomical work of the Observatory stand revealed in the impressive record of his photographic observations, which cover all phases of the Sproul astrometric program.

The Planets in May, 1948

By RAYMOND H. WILSON, JR.

NOTE: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. By the end of the month the sun will have reached 22 degrees north declination—nearly the summer maximum.

An annular eclipse will occur at the time of New Moon. It will be generally invisible in the United States, except that the beginning might be seen on May 8 just before sunset, northwest of a line from Cape Mendocino to the panhandle of Idaho.

Moon. The phases of the moon will occur as follows:

New Moon	May	8	8 P.M.
First Quarter		15	7 P.M.
Full Moon		22	7 P.M.
Last Quarter		30	5 P.M.

The moon will be nearest to the earth on May 15.

An occultation of η Leonis might be seen from the west coast states on May 15, shortly after 8:30 P.M. Pacific Time. Also in the same region one of γ Virginis might be visible on May 18, shortly after 6:30 P.M. Pacific Time.

Evening and Morning Stars. Venus, Saturn, and Mars will be conspicuous in the early evening; Jupiter later. This month is extraordinary in that all the major planets will be visible sometime in the evening sky.

Mercury. On the evening of May 28 this planet will be 23 degrees east of the sun. This is a very favorable eastern elongation since the declination of the planet will be 26 degrees north.

Venus. Greatest brilliancy for this planet will occur on May 18. A northern declination of up to 28 degrees will give it an unusually favorable position in the evening sky.

Mars. On May 22 Mars will be at quadrature, 90 degrees east of the sun and less than a degree north of Regulus in The Sickle.

Jupiter. This bright planet will rise before 10 P.M. by the end of the month. On May 24 the moon, just past full phase, will be in close conjunction with it to the south.

Saturn. The ringed planet will be at eastern quadrature on May 6, when it will be on the meridian at sunset. It will be just west of Mars in Leo, and moving eastward.

Uranus. Uranus will be moving slowly northeastward along the ecliptic at a point 2 degrees north-northwest of ζ Tauri.

Neptune. Neptune will be moving very slowly northwestward at a position 2 degrees southwest of γ Virginis.

Department of Mathematics, Temple University, Philadelphia, Pa.
March 1, 1948.

Asteroid Notes

By HUGH S. RICE

Since the last appearance of these notes, VESTA has been observed here by at least three telescopists, who saw it when between magnitudes 6 and 7. It is now on its way "out" and we shall have to wait for another apparition.

At present we have no ephemerides of bright minor planets to offer for observation. There are two 10th magnitude asteroids, 27 EUTERPE and 6 HEBE, however, that are well placed (in Leo) for observation with suitable instruments. We give the places below, as supplied by Dr. Dirk Brouwer, of the Yale University Observatory.

COMET BESTER. In the absence of bright asteroids we have been observing this comet from the Spuyten Duyvil section of New York. We first picked it up with Zeiss 50 mm binoculars on March 13, at $4^h\,30^m$ A.M., E.S.T. Identification was made instantly, as it was only necessary to *search near Theta Aquilae.* [This we mention because of the historic telegram which Klinkerfues of Göttingen sent to Pogson at Madras, on November 30, 1872. Professor Klinkerfues believed that Biela's comet might be seen, possibly, going away from the earth, and that observers should look toward the anti-radiant of the 1872 meteor shower. The telegram read: *Biela touched earth Nov. 27; search near Theta Centauri.* Curiously enough, a comet was found at this location, but Bruhns believed it could not be Biela's.] Comet Bester appears in our glass each time as a hazy star immediately seen, and with no tail. We observed it also on March 18, at $3^h\,30^m$ and on March 20, at 3^h when it was passing by the star Altair, and could be just brought into the same field of view with the star. Our place of observation was from indoors looking out; no better view was obtained by going outdoors. The comet's magnitude, 6 to 7, is considerably fainter than the ephemeris predictions.

ASTEROID EPHEMERIDES

For 0^h U.T., Equinox 1948.0

1948	27 EUTERPE α	δ	1948	6 HEBE α	δ
	h m	° ′		h m	° ′
Apr. 9	10 27.8	+12 17	Apr. 9	11 45.3	+17 8
14	10 27.6	+12 14	14	11 42.1	+17 28
19	10 28.1	+12 7	19	11 39.3	+17 42
24	10 29.2	+11 56	24	11 37.1	+17 50
29	10 31.1	+11 41	29	11 35.5	+17 53
May 4	10 33.6	+11 22	May 4	11 34.4	+17 51
9	10 36.7	+10 59	9	11 33.9	+17 44
			14	11 34.0	+17 32

Hayden Planetarium, American Museum of Natural History, New York, March 21, 1948.

Occultations for May, 1948

(Taken from the *Amerian Ephemeris*)

Date 1948	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION *a*	IMMERSION *b*	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION *a*	EMERSION *b*	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
May 14	4 Canc	6.2	1 28.9	−0.7	−1.4	96	2 26.9	0.0	−1.8	302
28	86 B.Capr *m*	6.2	7 34.8	−1.9	+1.3	65	9 4.1	−2.1	+0.4	256
31	290 B.Aqar	6.4	8 33.0	−1.4	+1.6	73	9 51.9	−1.3	+1.8	221
OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′										
May 14	ω Canc	5.9	0 46.6	−1.8	−0.5	80	1 48.0	−0.4	−2.4	319
14	4 Canc	6.2	1 20.1	−0.7	−2.1	129	2 23.7	−0.9	−1.2	271
21	λ Virg	4.6	1 11.1	−2.1	+2.8	67	1 46.9	+0.6	−2.8	3
28	86 B.Capr *m*	6.2	7 0.3	−1.3	+1.7	71	8 23.3	−1.9	+1.1	263
31	290 B.Aqar	6.4	8 8.9	−0.7	+1.8	70	9 22.7	−1.2	+2.0	235
OCCULTATIONS VISIBLE IN LONGITUDE +98° 0′, LATITUDE +30° 0′										
May 3	τ Aqar	4.2	10 7.1	−0.9	+1.4	80	11 22.3	−1.5	+2.1	228
14	ω Canc	5.9	0 46.3	−1.5	−1.8	121	2 0.4	−1.3	−1.2	281
21	λ Virg	4.6	0 52.2	−0.6	0.0	118	1 58.5	−0.8	−0.6	314
26	τ Sgtr	3.4	7 27.6	..	..	24	8 5.5	−1.6	−3.5	338
27	*A* Sgtr	5.0	5 43.9	−2.1	+3.6	32	6 24.2	0.0	−1.5	327
28	86 B.Capr *m*	6.2	6 39.1	−0.8	+0.9	97	7 56.4	−1.9	+1.7	244
31	290 B.Aqar	6.4	7 49.7	−0.4	+1.2	89	8 53.8	−0.9	+2.3	222
OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′										
May 16	η Leon	3.6	4 42.8	−4.1	+1.8	55	5 8.4	..	..	10
19	γ Virg *m*	2.9	2 48.3	−2.2	+1.0	89	3 45.2	−0.4	−2.5	351
25	48 G.Sgtr	6.3	11 5.0	−2.5	−1.1	117	12 20.9	−1.3	0.0	236
26	τ Sgtr	3.4	6 45.8	−1.7	+2.6	49	7 37.2	−0.4	−0.6	328

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to

the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

The annual report of the American Meteor Society is herewith submitted. In several respects it shows advances: in the number of persons observing, in the percent of meteors plotted, and in the fact that once more we are beginning to develop a few observers who work enough in a given year to attain the skill necessary for the derivation of good radiants. Also we are no longer handicapped by many of our younger members being in military service and hence, as a rule, being unable to observe. The increasing number of women who are taking an active part in observing is too a hopeful sign. The Hydrographic Office of the U. S. Navy is now publishing again in its Bulletin large numbers of fireball observations, and we find the daily press most cooperative when called upon.

Two Flower Observatory Reprints, No. 68 and No. 69, written in 1947, deal with meteors. The first contains the combined Meteor Notes for the year, the other is my second paper dealing with long-enduring meteor trains. This latter appeared in the Proceedings of the American Philosophical Society, Vol. 91, p. 315-327, 1947. Both reprints have been sent to all A.M.S. members who have paid their 1948 dues, and will be sent to others as their dues arrive. A large new supply of our recording sheets has just been printed and also of our meteor maps, some numbers of which had become too low for sending out all which were requested. There has been some discussion as to the wisdom of revising the meteor maps in the sense of making them somewhat larger and adding other small improvements. We are looking into this plan but, due to other pressing local matters, a decision has not yet been reached. In any case, it would be a year or two before such maps could be made available, so meantime the older ones will serve.

With warmer weather coming and also the half of the year when meteor rates go up, it is hoped that all our members will make due efforts to advance our aims during 1948 by active work. Many, in fact most, of our states do not have a single really active observer. Also we sadly need efficient regional directors in many sections. As mechanical devices for observing meteors will not lessen the value of naked-eye work, our members can be assured that their work really helps us to advance our knowledge in the field and hence should be continuously carried on. We cordially invite any person of either sex, interested in meteors, to write me on the subject of joining the A.M.S.

Last fall, in Meteor Notes, was published a paper sent me by the distinguished German authority on meteors, Prof. Cuno Hoffmeister. He has now submitted a second paper, dealing with an extension of the same ideas. This appears here as the second part of these Notes. Such papers furnish a most welcome variety to the routine form which perforce most of my own reports to the A.M.S.

must take, and they should be of interest and value to our members and many others.

In closing, may I make it clear that the publication of all data, in my hands up to this time last year, on long-enduring meteor trains does not mean that they are any less important for the future or that I do not as eagerly collect new data for an eventual third paper. Hence all readers are urgently requested to send me any such information which may come into their hands, either through actual observations by themselves or others, or by references to older accounts particularly those printed in papers in journals possibly unknown to me. Also I beg that all who publish papers dealing with any phase of meteoric astronomy will be kind enough to send me a copy of the journal containing it or a reprint, so that such papers may become a part of our growing A.M.S. library. Already our collection of such reprints must be one of the most extensive in existence.

Observer and Station	Nights	Meteors	Remarks
Anderer, J., Chicago, Ill.	1	56	C
Anderson, R. F., Chicago, Ill.	4	45	C
Anyzeski, V., New Haven, Conn.	9	116	P
Beetle, Mrs. D. E., Laramie, Wyo.	4	48	P
Bostick, Dr. J. F., Fresno, Calif.	4	94	C
Bufkin, Batavia, Ill.	1	144	C
Burn, E. H., Wallingford, Pa.	2	79	C*
Burns, H. A., Spring Valley, N. Y.	11	166	P
Bybee, G. F., Columbia, Ky.	26	324	P
Chabot, H. L., Bronx, N. Y.	7	107	P
Davis, R. J., Omaha, Neb.	5	68	P
de Kort, W., near Antwerp, Del.	10	184	P
Dole, R. M., Cape Elizabeth, Me.	7	264	P
Dow, J. P., East Aurora, N. Y.	2	36	P
Dupee, Mrs. D. S., Strafford, N. H.	6	224	D
Epstein, E. S., W. Yonkers, N. Y.	2	75	P
Federer, Mrs. H. S., Danbury, N. H.	1	75	C*
Fein, L., New York, N. Y.	1	25	PC
Fitzsimmons, S. C., Sibley, Iowa	7	146	P
Forsyth, E. L., Fall Brook, Calif.	6	114	D
Gibson, E. A., Wisconsin Dells, Wis.	1	285	C
Graves, Mrs. P. E., Saco, Me.	2	50	C
Griffith, Miss J., Sarasota, Fla.	1	28	P
Haas, W., Albuquerque, Arizona	1	21	P
Halbach, E. A., Milwaukee, Wis.	2	445	C*
Hartmann, Miss G. C., Lincoln, Neb.	1	85	C*
Hay, A. A., Chicago, Ill.	9	45	P
Hendricks, L., Hastings, Neb.	4	86	D
Hukill, R. M., Wilmington, Del.	4	70	P
Jensen, Miss A. K., Silverton, Ore.	6	557	C
Johnson, L. T., La Plata, Md.	2	60	P
Johnson, M. C., Des Moines, Iowa	5	23	P
Kennedy, Miss M. C., Sarasota Springs, N. Y.	3	26	D
Khan, M. A. R., Begumpet, India	45	838	P
Kingman, D., Holland, Mich.	3	215	C
Klass, N. P., Dubuque, Iowa	1	102	C*
Knowles, J., Marblehead, Mass.	7	331	C
Korn, K. W., Bronx, N. Y.	4	103	P*
Lane, F. O., Albuquerque, N. M.	1	21	D
Le Roy, S., New Haven, Conn.	1	25	PC
Le Roy, O., New Haven, Conn.	1	27	PC
Long, Mrs. B., Dell Rapids, S. D.	4	76	C
Macdrudd, D., Whitbey Island, Wash.	1	27	C

Observer and Station	Nights	Meteors	Remarks
McConarty, Miss E. M., Alberton, Mass.	1	21	C
McKeon, Mrs. J. J., Titusville, N. J.	2	58	D
Mains, M. M., Beatrice, Neb.	6	107	C
Martin, Miss P., Kearney, Neb.	1	20	C
Minglin, Miss M. K., Clear Lake, Wash.	1	33	D
Missert, R., East Aurora, N. Y.	2	39	P
Muzzalski, R. J., Port Blakeley, Wash.	1	33	P
Nailor, C. D., Norristown, Pa.	10	128	P
Nailor, Miss N., Port Angeles, Wash.	1	22	P
Nangle, J. E., Peoria, Ill.	2	75	C
Nat. Capital Astr., Washington, D. C.	4	49	C*
Neale, J. J., New Haven, Conn.	1	28	C
Oberst, W., Pasco, Wash.	1	44	C
Olivier, C. P., Flower Observatory, Pa.	1	45	PC*
Oravec, E., New York, N. Y.	9	90	PD
Park, R. E., Cleveland, Ohio	4	17	P
Peabody, J., Batavia, Ill.	1	159	C
Pierce, L. W., Elgin, Ill.	1	152	D
Posten, H., New Britain, Conn.	2	72	P
Preucil, F., Joliet, Ill.	2	298	C*
Pruett, J. H., Eugene, Ore.	1	37	C
Rayna, G., New York, N. Y.	3	43	P
Richardson, T., Norman, Okla.	9	167	P
Roberts, Miss C., Rippey, Iowa	1	31	C*
Rutkowski, E., Trenton, N. J.	5	110	P
Scott, T., Nauvoo, Ala.	54	1404	P
Schilling, J. H., Fort Bragg, N. C.	2	14	P
Schmidt, L., Geneva, Ill.	1	17	D
Skumanich, A., Wilkes-Barre, Pa.	7	172	PC*
Siekman, W. J., Batavia, Ill.	15	382	P
Skinner, G. B., Brockton, Mass.	1	24	P
Sky Scanners, New York, N. Y.	1	37	D*
Snyder, F. H., Cincinnati, Ohio	1	46	C
Stevenson, C. B., Little Rock, Ark.	2	91	D
Stowe, B., Yonkers, N. Y.	4	119	P
Swihart, T., Elkhart, Ind.	6	103	P
Taleperos, A., Levinson Manor, N. Y.	3	60	D
Thomson, Mrs. H., Eugene, Ore.	1	46	C
Thomson, Miss B. J., Eugene, Ore.	1	54	C
Turnbull, H. S., Seal Harbor, Me.	5	31	P
van Kleeck, E. S., Coronado, Calif.	2	30	C
Velez, A., Mayaguez, P. R.	1	35	C
Waldmann, E. G., Brooklyn, N. Y.	22	155	P
Walker, G. B., Williamstown, Mass.	2	63	C*
Weitzenhoffer, K., Bronx, N. Y.	1	119	P
Whitney, B. S., Norman, Okla.	1	105	C*
Widner, D., Portland, Ore.	4	143	DC
Wood, R. J., U. S. Navy	4	42	P
Wright, M. G., Plevna, Ind.	3	82	P
16 observers with less than 20 each		154	C
Miscellaneous reports		34	
Fireballs (not in routine reports)		126	
Telescopic meteors		82	
Total for 1947		11,584	

In the Remarks column, P means the meteors were plotted and recorded singly, D that they were singly recorded but not plotted, and C that timed counts were made only. An * means that more than one observer's observations go to make up the number of meteors reported.

Flower Observatory, Upper Darby, Pa., 1948 March 5.

Meteoric Currents of the Cygnid Type

By C. Hoffmeister, Sonneberg Observatory

In addition to my paper entitled "The Ecliptical Meteoric Currents" (Popular Astronomy, **55**, 492, 1947) I give here a second communication referring to an extension of the System of Ecliptical Currents recently established. After the accomplishment of my general work on meteoric currents there remained to be investigated several currents, which might be regarded as of cometary origin, but having no parental comet so far known. Their radiant points are situated at relatively high ecliptical latitudes, so that, in spite of the very long duration of activity, no relationship to the Ecliptical Currents could be anticipated. There are three systems of this kind: the *Cygnids,* active from July 25 to Sept. 8 with a very flat maximum by Aug. 16, the contemporaneous current of the *Cepheids,* being probably a mere branch of the Cygnids, and the *Velaids,* a southern current observed between Dec. 5 and Jan. 7 with a hardly perceptible maximum about Dec. 29. There arose also some doubts as to the character of several other currents of secondary significance, especially the Hydraids in March, which current might be regarded as a branch of the contemporaneously active Virginids, the only obstacle being the relatively high latitude of the radiant point of —23°. Thus the interest concentrated on the two systems of the Cygnids and the Velaids.

The subsequent investigation, based on the methods briefly described in the paper mentioned above, yielded the surprising fact that both of these currents agree as to their general character closely with the Ecliptical Currents, their orbits being ellipses of short time of revolution, moderate eccentricity, and semi-major axes between 1 and 2 astronomical units, but with the essential difference of large inclinations to the fundamental plane of the Solar System.

In the following table are given three systems of elements for each current, the principal orbit corresponding to the maximum of activity, and two limiting orbits corresponding to the extreme wings of the current, thus showing the ranges of the variation of elements during the whole extent of the current's activity.

Cygnids

		First orbit	Principal orbit	Last orbit	
Argument of perihelion	ω	53°31′	70°35′	93°16′	1925.0
Ascending node	Ω	121 18	142 10	162 36	
Inclination	i	44 49	46 22	55 40	
Eccentricity	e	0.4585	0.5232	0.6377	
Semi-major axis	a	(1.637)	1.637	(1.637)	
Perihelion distance	q	0.886	0.780	0.593	

Velaids

		First orbit	Principal orbit	Last orbit	
Argument of perihelion	ω	344° 1′	7°30′	20°47′	1925.0
Ascending node	Ω	75 12	97 0	106 30	
Inclination	i	62 50	70 31	79 16	
Eccentricity	e	0.1538	0.1518	0.1584	
Semi-major axis	a	(1.158)	1.158	(1.158)	
Perihelion distance	q	0.980	0.982	0.975	

Disregarding the high inclination, the orbit of the Cygnids is in full agreement as to its character with the orbits of the 6 Ecliptical Currents, especially

the value of the semi-major axis being within the rather narrow limits established by those currents. A slight systematic difference may be indicated by the smaller value of eccentricity which, in the author's opinion, might be taken as real. The still much lower value of the Velaids, however, must be regarded as doubtful, the observations being a little too scanty as yet in the case of this current.

The fact that there are meteoric currents possessing all the properties of the Ecliptical Currents, but having high inclinations of their orbits towards the principal plane of the Solar System, proves to be, at a closer inspection, not so surprising as it may appear at a first glance. The conception of a very flat shape for the Solar System is only valid for its major bodies but neither for the Minor Planets nor for the reflecting matter causing the Zodiacal Light. As to the Minor Planets this fact became obvious at the very beginning of their exploration, the second of those bodies in the sequence of discoveries, Pallas, exhibiting an inclination of nearly 35°. Indeed among 1546 Minor Planets listed so far there are 96 with inclinations exceeding 20°, and 9 with inclinations exceeding 30°. The maximum value known is 42°.5 belonging to the planet 944 Hidalgo, Pallas occupying the third place in the present list. Also the matter of the Zodiacal Light has been found, from photometrical investigations, to be spread to a considerable distance from the principal plane of the system in its inner regions.

The Ecliptical Currents in the proper sense and the group of the Cygnid Type Currents are thus forming a system of *Planetary Meteors* in close agreement with the distribution in space of other minor bodies of the Solar System, where an intermediate position may be ascribed to the meteoric particles between the Minor Planets and the dust particles of the Zodiacal Light.

As to the number of observed meteors the share of the Cygnid Type Currents is scarcely 10 percent of that of the Ecliptical Currents, approximately the same ratio as between Minor Planets of large and of small inclinations. The total share of the Planetary Meteors, referring to the total of observed meteors thus amounts to 18 percent, and with 13 percent of Cometary Meteors detected by other investigations, the share of the Interstellar Meteors would be 69 percent.

Concerning the physical appearance of the meteors, both Cygnids and Velaids are in good agreement with the Ecliptical Currents, differing strongly from cometary meteors. The notation of the following table may be taken from, and the values given compared with, the adequate table in POPULAR ASTRONOMY, **55**, 496.

Current	$\overline{M}$	X	F	T
Cygnids	3.71	0.81	1.31	30%
Velaids	3.93	0.98	1.30	27%

As may be seen from the table of elements given above the system of the Cygnid currents includes the value $\omega = 90°$, thus indicating that the current encounters the Earth in both nodes. The apparent radiant in the ascending node after perihelion is, for $\odot = 340°$, $\alpha = 332°$, $\delta = -70°$. This point being, early in March, near lower culmination at midnight, the visual observation of the return current is confined to higher southern latitudes. The elongation of the apparent radiant from the Sun is 63°.

The orbit obtained for the Cepheids differs but little from that of the Cygnids; so that current might indeed be regarded as a mere branch of the lat-

ter. The Hydraids proved to be a branch of the Virginids as suggested above, showing the general tendency to widely scattering and the formation of secondary currents, as has been found before in the case of the Scorpius-Sagittariids, and in the relationship of the Piscis-Austrinids to the δ Aquarids.

Errata in November, 1947, POPULAR ASTRONOMY: Page 493: The eccentricity of Sco-Sgr-System is 0.7339 instead of 0.9339. On page 495, last word of the text, read *attitude* instead of *altitude*.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD

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A Proposed Sky Patrol by the Meteoritical Society*

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ABSTRACT AND INTRODUCTION

In these days of international tension, our country should have a means of recognizing promptly authentic reports of V-2 bombs, high-speed planes, or bomb-carrying balloons seen in the air. A report of such an object would be lost now in the mass of material which is ignored or investigated by persons without proper training. The present failure of scientific men to explain promptly and accurately flaming objects seen over several States, "flying saucers," and other celestial phenomena which arouse national interest is causing the public to lose confidence in the intellectual ability of scholars. The mass hysteria over simple, but unexplained, phenomena is bad for the morale of the country. Many of these reports refer to detonating and stone-dropping meteors, and information on these would be of great value to astronomy and meteoritics.

Work at Iowa covering a limited region in the Middle West for certain summers shows that getting the facts back of these reports is not difficult for the specialist, and that a year-round coverage of the entire United States would not be expensive. The setting up of such a nation-wide "patrol" by the Meteoritical Society is recommended.

The student working on small meteors, or "shooting stars," can have trained observers record the paths as plots on star charts, by the coördinates of a reticle or by photography, and get plenty of useful data for each hour expended, but the average meteor worker has never seen a detonating meteor, one of the spectacular objects which cast strong shadows and seem to turn night into day momentarily. We cannot expect to use photography, or trained observers, for these; but such a meteor falling at night attracts the attention of everyone who is outdoors and away from lights in an area of nearly a million square miles

*Presented before the meeting of the Section on Astronomy (Section D) of the American Association for the Advancement of Science, at 2:00 P.M., on Friday, December 26, 1947, in Chicago, Illinois (114th Meeting of the Association).

(with clear weather, of course). We can, therefore, expect to find, for each such meteor, plenty of intelligent persons who have observed it under favorable conditions. A trained interviewer arriving on the scene promptly will get, from these persons, results as much superior to the material in the usual letters and published reports, as is the physician's diagnosis of an illness superior to the guess of a neighbor's wife.

In the course of several years' work, concentrated in the summer months, when we were free of teaching, and covering effectively only 3 or 4 States, we developed a program which could easily, with sufficient financial support, be expanded to a nation-wide and year-round coverage.

For the first notifications we depend primarily on representatives of the press, such as those of the Associated Press. Editors and coöperating workers in the vicinity of the meteor's fall are then "contacted" by long-distance telephone, and the names of observers of the meteor obtained. A few of these in key points are then "contacted" directly by telephone, enough to fix the path and circumstances of the fall of the meteor with all the accuracy desired by the average newspaper reader, and with sufficient accuracy for our own preliminary work. A trained interviewer then visits a number of observers to measure the angles and obtain the other data needed for the accurate path thru the atmosphere and the orbit about the Sun.

As soon as we embarked on this program of work on spectacular meteors, we were notified whenever a person saw anything conspicuous in the sky which he did not remember having seen before, or whenever he found something which he thought might have fallen from the sky. Checking these reports takes more time than might be expected, as more than 90% of the press stories on freshly found meteorites are discovered to refer to rocks or other common terrestrial objects.

To illustrate the type of reports received: requests that search be made for an airplane going down in flames, coming from points 300 miles apart, have been found to refer to a meteor 200 miles from the nearer of those points. A report that an airplane had fallen in flames into Lake Michigan, and a report of a meteor burning limbs from a tree, 400 miles away, were found to refer to the same meteor. Reports of meteors cutting power lines, or setting fire to straw stacks, barns, and houses have been found to refer to no meteor, or to a meteor a hundred or more miles away. The flaming object which crashed near the Texas-Mexico border on October 12th of this year (1947) was reported by Mexicans as a V-2 bomb, but investigation by a member of the Meteoritical Society showed that it was a meteor.

Perhaps the most successful scientific hoax of the last 50 years was the story sent out in 1930 from Crawfordsville, Indiana, of a meteor going thru the hood and radiator of a car. This story was featured with photographs in *The Literary Digest, Popular Science Monthly, Pathfinder, Science and Invention,* Sunday supplements, and most daily papers. Our investigation showed that the holes had been made by a shotgun. The perpetrators of the hoax confessed, *after* the investigation, of course.

Many interesting stories of "something" seen in the sky are not thought by anyone to refer to meteors; *e.g.*, when near the horizon at night, the bright planet Venus resembles the light of a not-too-distant plane; and, for weeks at a time, Venus can be seen in broad daylight by persons with normal eyesight. On one

occasion, a young man in Storm Lake, Iowa, found it in daylight, and had a considerable number of the people of the community looking at it. On another occasion, the offices of Wall Street, in New York City, were nearly emptied temporarily to look at Venus in the daytime sky. On both occasions people thought that they were looking at a comet and that they should be able to see it, "tail and all." On numerous occasions during the recent war, Venus was mistaken for an enemy craft of some sort, and several stories of aviators, going up to shoot it down, have been received.

The crescent Moon in the daytime sky is another common celestial object which is unfamiliar to many. The convex side is often up in the daytime. The Moon then resembles a parachute, and it is occasionally mistaken for a bomb-carrying parachute.

An unfamiliar object in the sky which is not explained may start talk and develop into community hysteria, as in an Iowa community while the Japanese bombs were coming over. A trained person would have found from a few interviews whether the object or objects seen were about 100 feet high, about 5,000 feet high, or at the distance of the stars and planets, but, some weeks after official investigators had arrived on the scene, we received a letter from the county sheriff. He wanted us to give the position of the planet Venus in the sky in the early evening and later, so that he could publish a note in the local paper and calm the fears of the people in his community.

The "flying saucers" are a more recent example of mass hysteria, and on a national scale. The first reports were not investigated, as far as we know, but they suggest that certain pilots saw what we, and no doubt many others, have seen. In driving west in the morning hours, if an airplane crosses the road some distance ahead, the sunlight reflected from its windows may obliterate the outline of the plane, giving the appearance of a round or oval and brilliant spot of light moving about in the sky. Later reports referred obviously to various other things, and even to tricks played by pranksters. One which we investigated in a nearby village referred to glowing bits of paper drifting frome a fireplace chimney. In another instance, a saucer was reported as passing over, the height having been estimated as 5,000 feet by a former pilot. A few interviews showed that the object was only 250 feet high and obviously something carried by small balloons. On the next day, some young men confessed that they had sent up a big paper saucer carried by helium-filled balloons.

The present national policy is to ignore reports of objects seen in the heavens. The press may publish an occasional guess by a man with scientific training, but there is no provision for finding the real facts back of these reports. We believe that our citizens have plenty of real troubles without adding the burden of imaginary dangers, and that, in these days of robot planes, bomb-carrying balloons, V-2 bombs, and atomic bombs, neither individuals nor communities should be allowed to become hysterical because of our failure to explain some simple phenomenon, well-known to us, but not understood by the general public.

We believe that scholars cannot hope to hold the respect of the public if they fail to explain simple phenomena in which people in general are interested. Scholars should not allow newspapers from coast to coast to carry such headlines as "Mysterious Flaming Object Over Middlewestern States Puzzles Scientists," when a few telephone calls would give all the data necessary for a good press feature. Our failure to handle better the various stories during the war,

and the "flying-saucer" stories after the war, has no doubt contributed to a lowering of respect by the general public. It has been said that a civilization which does not respect intellectual ability as much as athletic prowess is on the road to decay. Certainly scholars cannot hope to exert effective leadership if they are not respected.

In these days of international tension, it seems that, as a matter of national security, we must be ready to recognize authentic reports of V-2 bombs, robot, highspeed planes, or bomb-carrying balloons, seen in the air; it is obvious that we should not now. Such a report would be lost in the mass of material which, at present, is completely ignored or investigated only by persons without the technical training essential for useful work.

Finally, of considerable interest to astronomers, but less to others, is the fact that a number of questions in meteoric astronomy listed in present textbooks as unsolved will be decided when, and only when, accurate paths and orbits are determined for a number of these spectacular objects; and, as a by-product of this work, additional data on the resistance of the air to bodies traveling at highly supersonic speeds will be obtained.

As was stated earlier, the test work carried on at Iowa in certain summers could easily be expanded to cover the nation for the entire year. In view of the obvious need for this work, we are recommending that a national program of this type be instituted. Our summer work has shown that the covering of 3 Middlewestern States effectively required 2 full-time men and a secretary, plus volunteer workers scattered over the area covered. To cover the nation effectively we are suggesting a national director, several regional directors, and a considerable number of volunteer workers. As volunteer workers we could expect to have eventually a science teacher in practically every college and high school and a number of coöperative observers of the U. S. Weather Bureau. These are the persons to whom questions on celestial phenomena naturally come when there is no astronomer in the community.

The Meteoritical Society could furnish the regional directors, who would be mostly college science teachers. Each region should be small enough for the director to cover it without help beyond that which his college or institution can furnish.

The national director should have, to help in his office, an astronomer, a psychologist, and whatever technicians, computers, and secretaries are needed to make the work effective for the purposes of national defense. From our previous work, however, we feel that the cost of the entire project need be no more than the cost of a good university laboratory for research in some field of science.

December 23, 1947

Sampling Theory Applied to Meteoritic Populations

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ABSTRACT

Meteoriticists have long believed in the existence of areal anomalies in the horizontal distribution of meteorites. On the one hand, the significantly different proportions in which iron and stone meteorites are found in samples of the *total* meteoritic populations of the eastern and western hemispheres have repeatedly

called forth the statement that such an anomaly, having an origin somehow connected with geographical location, exists between the hemispheres. On the other hand, Högbom has conjectured that a similar areal anomaly, having its origin not in geographical location, but rather in the local selective depletion of native-iron meteorite-populations by the agency of man is found on comparing the meteoritic populations of the Old (or long-inhabited) World and the New. This second partition of the globe into subregions is distinct from the separation into eastern and western hemispheres.

The present paper critically subjects the evidence for these suggested anomalies to analysis, by use of the sampling theory developed in the study of discrete, 2-fold populations. Such an approach is possible only because of the recent publication by Frederick C. Leonard and Boris Slanin[17] of invaluable compilations relating to the areal distribution of all recovered meteorites. On the basis of the exhaustive sampling data thus supplied, the following conclusions have been reached: Denote by W a meteoritic population derived from witnessed falls and by U such a population derived from unwitnessed falls; then no significant difference as regards the incidence of stony character exists between (1) the W's of the eastern and western hemispheres; (2) the U's of these hemispheres; (3) the W's of the Old and New Worlds; and (4) the U's of the same subregions. The analysis carried out makes clear also that current belief in areal anomalies stems from the logically indefensible practice of lumping together the U and W populations of each of the distinct geographical areas compared.

Introduction.—Peculiarities in the observed horizontal distribution of recovered meteorites have long intrigued meteoriticists. On the one hand, we have such anomalous *local* concentrations as that in the southern Appalachians,[1] for which a satisfactory explanation seems to have been given first by the writer;[2] and, on the other hand, apparent *areal* anomalies of vast extent, such as the quite different proportions in which irons (including pallasites) and stones have been recovered in the eastern and western hemispheres. As long ago as 1915, Farrington[3] pointed out that, of the 256 meteorites then known from the western hemisphere, "182 are irons and only 74 stones, while from the eastern hemisphere, of 378 known, 299 are stones and only 79 are irons." Farrington, in reaching the conclusion stated, lumped together recoveries from both witnessed and unwitnessed falls, thereby setting a precedent for the use of an illegitimate procedure which, unfortunately, has been widely adopted by his successors. Since Farrington's comment was published, many writers have ventured the opinion that the difference to which he referred in the proportions in which irons and stones have been found in the 2 hemispheres is "very difficult to explain."[4] The natural result of such statements has been the publication of a variety of attempted explanations, some quite heuristic, like Högbom's suggestion[5] that the preponderance of iron meteorites (plus pallasites), with respect to the stony meteorites in North America (69%), Central and South America (75%), Australia (81%), and Siberia (77%), in contrast to their dearth in Europe (14.2%), India (2.8%), and Japan (12.5%), results from the fact that the countries of the latter group have long been inhabited by peoples sufficiently advanced technically to be able to work, smelt, and otherwise to destroy a large number of the iron meteorites

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[17] This and other references will be found at the end of the completed paper.

originally present in their lands; some merely curious ones, like Hinks'[6] singular suggestion that not all of the disproportionate number of irons found in the U.S.A. are "genuine meteorites"!

It would appear that before elaborate attempts are made to "explain" the observed differences in the stone/iron ratio, it should be established that these differences are actually significant and not the result merely of chance. Anyone who has ever flipped a coin will have been struck by the occasional appearance of so-called "runs," *e.g.*, the appearance of 5 successive heads in a series of 20 tosses. To those unfamiliar with the elements of mathematical probability, such runs appear to be significant. Actually, the occurrence of runs—even ones of arbitrarily great length—is predictable on the basis of the calculus of probability.[7] Specifically, this theory shows that, on the average, about once out of 4 times, a series of 20 tosses of an unbiased coin will exhibit a run of 5 heads; hence, the occurrence of such a difference in the proportion of heads and tails, as a run like this generally confronts us with, is quite without significance, in spite of popular opinion to the contrary.

The purpose of the present paper is to investigate observed differences in the proportions in which stones and irons appear in populations of meteorites, reasonably homogeneous in the statistical sense, recovered from various subregions on the Earth, by use of the theory of probability, in order to determine whether or not these differences are actually significant. To legitimatize our use of this theory on the meteorite samples chosen for study, we must first verify that these samples have been collected under conditions at least approximately satisfying the postulates on the basis of which the mathematical theory was developed. These are, first, the postulate of *randomness* in the sampling procedure, according to which each member of the population investigated has the same chance of being chosen; and, second, the postulate that the so-called "success probabilility," p (*e.g.*, the probability that, when a meteorite is selected at random from the totality of meteorites in a certain region, it will be a stone) remains *constant*.

Before attempting to verify that the meteorite samples which we shall select for study do approximately satisfy the basic postulates of sampling theory, it is essential to point out wherein Farrington and his successors erred in lumping together recoveries from both witnessed (W-) and unwitnessed (U-) falls. Briefly, such a procedure is illegitimate, because it definitely violates the first of the postulates just stated. Such violation arises in at least 2 ways: In the first place, even under the assumption that the relative frequency of irons and stones is the same among W- as among U-falls, it is obvious that the individual falls in the class U do not have the same chance of being discovered (and of thus representing this class in meteorite collections) as do the individual W-falls. This fact results, not alone from the existence of *positive aids* to the discovery of W-falls, provided by the impressive light and sound phenomena accompanying them, but also from the existence of such *deterrents* to recovery of U-falls as the weathering away and disappearance not only of all superficial evidence (such as impact craters, penetration funnels, broken limbs, bark scars, *etc.*) of the occurrence of such falls, but also of the fallen material itself. In the second place, the hypothesis that the ratio of irons to stones is the same in the W- and U-classes is itself illegitimate, for it rests on 2 implicit assumptions which need only to be explicitly stated to be recognized as quite indefensible.

The first of these is the assumption that the proportion of irons to stones observed for *W*-falls at the present time correctly represents the value of the iron/stone ratio thruout the immense period of time during which meteorites have been accumulating to form the *U*-class; and the second is the assumption that the irons and stones are equally and perfectly resistant to such natural destructive processes as weathering, and that their relative proportions in the *U*-class have not been altered during the course of time by such things, as, *e.g.*, selective depletion of the population of iron meteorites by human agencies. Having in mind the numerous and cogent reasons just advanced for criticizing Farrington, we shall most carefully avoid lumping together meteorites recovered from the statistically distinct *U*- and *W*-classes in everything that follows.

Because in even the smallest of the countries to be considered in the sequel, the population of meteorites of witnessed and that of unwitnessed falls are both very large, it follows that, to a very high degree of approximation, the second of the foregoing postulates is satisfied by the meteorite-collecting processes, whereby samples of these respective populations have been selected, in spite of the fact that when any meteorite fall is discovered, it is withdrawn from the population sampled.

The sampling process for *W*-falls evidently satisfies the first basic postulate of sampling, but, at first sight, it may appear that this postulate is not even approximately satisfied by the discovery process operating to produce samples of *U*-falls. Closer study, however, brings to light a number of compensating factors which operate to insure a fairly high degree of randomness in the sampling of *U*-falls; *e.g.*, a small stony meteorite of a *U*-fall may seem to have a disproportionately tiny chance of being discovered at all, and, therefore, of entering into one of the *U*-samples to be subjected to mathematical analysis. Such a meteorite is, however, almost without exception, only one member of a shower of similar meteorites, the discovery of any one of which will effectively include the particular stone fall in the *U*-sample. Clearly, the smallness of the probability of discovery of a particular *individual* of such a shower fall is offset by the very large probability that *some* member of this shower will be found and thus insure entry of the fall into our *U*-sample. Again, because of their great density, iron meteorites penetrate to exceptional depths, thereby minimizing their chances of discovery, but precisely their high specific gravity brings into play a compensating factor, for it suffices to endow the siderites with a "heaviness" proclaiming them, even to the least observant, as remarkably different from ordinary rocks, and therefore as meriting collection and preservation. Many other similar compensating factors will occur to the reader. Taken *in toto*, they appear to guarantee that the *U*-samples studied later in this paper satisfy, at least approximately, even the first of the basic postulates of sampling theory. Support for this view is found in the fact that the only experimental tests, analogous to sampling of a *known* meteoritic population of *U*-falls, so far carried out (those conducted by the writer in his study of criteria for estimating the populations of meteoritic showers[8]) gave results in conformity with the assumption that the discovery of the small fallen meteorites used as test objects was a random process. That such empirical evidence is not to be regarded lightly is shown by the following statement in regard to the practice followed by statisticians in attempting to justify that a given sampling process possesses the characteristic of randomness[9]: "All that can be done, and all that has been done, is to apply to a *known* popula-

tion some apparently random method and to compare the results obtained with those expected upon the basis of probability theory. If the actual and theoretical results agree reasonably well when applied to the known population, it is concluded that the given method will yield random samples also when applied to an *unknown* population."

Additional justification for employing the theory of simple sampling in the study of meteorite samples, can, of course, be found in the fact that it has been successfully applied to other types of samples, quite as evidently failing *exactly* to fulfil both postulates basic to the theory as do the samples of *U*-falls; *e.g.*, the samples supplied by the innumerable polls to which the inhabitants of every large city are now subject, exactly satisfy neither the postulate of randomness nor that of the constancy of p: a man once questioned as to what candidate he plans to support is not returned to the population to be sampled (for he will not be interrogated again), and hence p does not remain constant during the polling process. (Just as in the meteorite case, this discrepancy is of negligible moment because of the very large populations from which the samples are taken.) Again, the supposition that all the inhabitants of New York are equally likely to be reached for interrogation is palpably absurd; in fact, it would seem that the randomness postulate can scarcely be much more closely fulfilled in public-opinion polls than in the case of discoveries of *U*-falls; nevertheless, as every one knows, astonishingly accurate predictions have been made on the basis of such polls, by use of the sampling theory.

§1. *The Sampling Theory.*—Each meteorite discovered is either iron or stone. Hence, we are concerned with a study of samples drawn from a discrete population, every element of which is assignable to one or the other of 2 categories. As is well known, the mathematical tool used in the investigation of samples from such a discrete 2-fold population is the theory of the *binomial probability-distribution*[10] developed to analyze the results obtained in a series of n independent trials with constant probability of success, p, such as the coin-tossing problem considered in the introduction, in which each toss constitutes an independent trial, the probability p of success, *i.e.*, of getting a head, being constantly equal to ½. The data supplied by n tosses constitute a sample analogous to that supplied by a collection comprizing specimens of n distinct meteorite falls. In the latter case, however, we have no *a-priori* means of evaluating p (now the relative frequency of stony meteorites in the population from which our collection was drawn), and we must therefore estimate the value of p from the sample itself.[11]

For the binomial distribution, the expected or mean value of the *number* ν of successes in a series of n trials is $E(\nu) = np$; the variance of ν is $np(1-p)$; and, therefore, the standard deviation of ν is $\sqrt{np(1-p)}$. The *proportion* π of successes (stones) in a sample containing n meteorites is simply the ratio of the number of stones in the sample to n. The expected or mean value of this proportion is therefore $E(\pi) = p$, and the standard deviation of π is

$$\sigma = \sqrt{p(1-p)/n}.^{12}$$

Let now 2 large meteoritic populations, M_1 and M_2, be tested for the frequency of occurrence of stony meteorites, by taking from them large samples (collections) of n_1 and n_2 meteorites, respectively. Let $\pi_1 = \nu_1/n_1$ and $\pi_2 = \nu_2/n_2$

denote the *observed* proportions of stones in these collections. In general, as the evidence presented in the introduction shows, π_1 is not equal to π_2, and our meteoritic sampling problem may now be precisely formulated as follows:

Does the occurrence of a difference $\Delta = \pi_1 - \pi_2 > 0$ *indicate a real difference between the meteoritic populations* M_1 *and* M_2 *as regards the incidence of stony character?*

To answer this question, we first introduce the hypothesis (H), that the observed difference Δ is *not* significant, *i.e.*, that M_1 and M_2 are similar as regards stoniness. Now under (H), we may lump our 2 samples to obtain the following estimate of the relative frequency of stoniness in the combined meteoritic population, $M_1 + M_2$, namely,

$$p = (\nu_1 + \nu_2)/(n_1 + n_2). \tag{1}$$

Our earlier formula for σ shows that the standard deviation of π_i is

$$\sigma_i = \sqrt{p\,(1-p)/n_i} \qquad (i = 1,\ 2), \tag{2}$$

where p has the value (1). Furthermore, since the samples are *independent*, the variance σ_Δ^2 of the difference Δ is given by[13]

$$\sigma_\Delta^2 = \sigma_1^2 + \sigma_2^2 = p\,(1-p)\,[(1/n_1) + (1/n_2)]. \tag{3}$$

Finally, we note that, under our hypothesis (H), the expected or mean value of Δ is zero for[14]

$$E\,(\pi_1 - \pi_2) = E\,(\pi_1) - E\,(\pi_2) = 0. \tag{4}$$

In order to simplify our later calculations, we next make use of the well-known fact that, for large values of n, the binomial distribution, whether symmetric or not, approximates the familiar *normal* or *Gaussian* distribution.[15] Thus, when n_i $(i = 1, 2)$ is large, the sampling distributions of ν_i and therefore of π_i are approximately normal; but, then, since our 2 samples are independent, the same fact is true of the distribution of the differences Δ.[16] Hence, finally, we conclude that the sampling distribution of the differences $(\pi_1 - \pi_2)$ is an approximately normal distribution with mean zero (by (4), *ante*), and with a standard deviation, σ_Δ, derivable from (3), *ante*. We are therefore in the advantageous position of being able to make use of the normal probability table in our efforts to solve meteoritic sampling problems of the sort formulated earlier in this section. The final step in the solution of each such problem will be to test the credibility of our hypothesis (H) by computing from the normal probability tables the probability, P, for a difference $(\pi_1 - \pi_2)$, numerically larger than the observed difference Δ, to occur. If P is sizable, the observed Δ is not significant; *i.e.*, it affords no grounds for doubting the validity of our hypothesis (H). On the contrary, if P is very small, then this difference Δ is significant and provides evidence against the hypothesis (H). While there is no hard and fast line separating values of P regarded as sizable from those regarded as small, nevertheless, it is the universal practice among statisticians to regard differences Δ as first becoming significant when they are so large that the probability of numerically greater deviations is *less than* P, where P is not greater than 0.05 and frequently is required to be only 0.025.

[*To be continued*]

President of the Society: Arthur S. King, Mount Wilson Observatory, Pasadena 4, California

Secretary of the Society: Oscar E. Monnig, 1010 Morningside Drive, Fort Worth 3, Texas

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

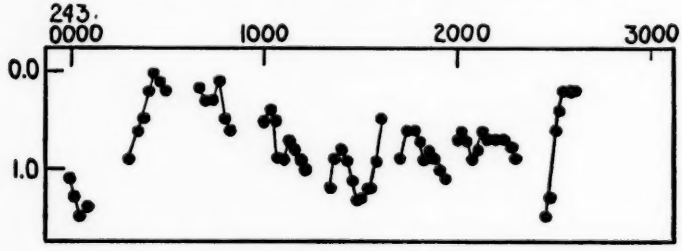
By LEON CAMPBELL, Recorder

Alpha Orionis: The bright red variable, α Orionis, is now at what appears to be another bright maximum. The star is difficult to observe, not only because of its redness, but also due to the lack of well-placed comparison stars. Aldebaran (magnitude 1.06) proves to be the most suitable for use during most of the time when the variable is not too bright. It is well known that α Orionis has a greatly extended atmosphere; interferometer measures show that it is subject to variations in diameter, and it has a variable radial velocity.

The question of its period has been a disputed one ever since the discovery of the star's variability by John Herschel over a century ago. Values ranging from 70 days to a year have been given for its short-term period, and a long-term period of just under six years has been indicated also.

Probably the most reliable series of observations obtained of this star are those made in 1908-1931 by Stebbins and his co-workers, using the selenium and photoelectric cells. These accurate observations indicated a long-term period of 5.78 years, comparable with the value of 5.40 years, derived by H. Spencer Jones from a study of the spectroscopic data. The period of 5.78 years does not remain constant, however, as is shown by a discussion of the earlier visual observations.

In order to show how well ordinary visual estimates of such a red star can be made by amateur observers, a plot of the light curve as deduced from AAVSO observations extending over the past seven years is shown in the figure. The



α ORIONIS (1941-1948)

plotted points are given to the nearest tenth of a magnitude only, and have been read from ten-day mean curves. The overall visual range appears to be about a magnitude and a half (0.0 to 1.5), whereas the photoelectric range is only a magnitude, but the scatter among the visual estimates is unusually large, as would be expected. A well marked long-term maximum is indicated in 1942, as well as the one at the present time. That there is a short-term variation as well is also indicated, and this agrees with the photoelectric results as obtained by Stebbins. The Gaposchkins class the variable among the semi-regulars, which class includes many stars showing double periods—of which V Hydrae is a

typical example—and varied forms of light curve. This star should be ideal for measurement with the latest forms of photoelectric photometers.

Zeta Aurigae: In view of the recent interest shown in this eclipsing star, Dr. Zdenek Kopal supplies the following note.

In December, 1947—January, 1948, astronomers were once more given the chance to observe the minimum of the well-known eclipsing variable ζ Aurigae, due to the eclipse of its early-type component of spectral class B by the principal component which is a K5 supergiant. The orbital period of this remarkable system is 973 days or little less than three years. It so happens, however, in recent decades that, out of three successive minima, two occurred at a time when the constellation of Aurigae is not far from conjunction with the sun, so that only each third minimum can be properly observed during the night time. Owing to a great difference in the spectral types of the two components, the amplitude of the light changes of ζ Aurigae during the eclipse is strongly frequency-dependent, amounting to about one-tenth of a magnitude in accessible infrared and increasing progressively to two magnitudes or more if we go sufficiently far in the ultraviolet.

The last minimum which could be well observed took place in 1939. The two subsequent minima in 1942 and 1945 passed almost unnoticed because of the unfavorable position of the constellation and because most astronomers of the world then had other preoccupations. Great hopes were, therefore, attached to the minimum of 1947-48 which took place in the recent couple of months. According to the best prediction which could be made, the eclipse should have begun some time on December 14, 1947, and within approximately 24 hours the brightness of the system should have dropped to a minimum lasting about thirty-nine and a half days. Thereafter the B star should have reappeared just as rapidly as it disappeared little more than a month before, so that after January 23, 1948, the star should have been again in full light. As far as it is known at present—in contrast to the last favorable minimum of 1939-40—no photometric observations of the ingress or egress of the eclipse have appeared. At the Harvard College Observatory—where the skies were partially clear during the critical night of the expected ingress—Dr. Stefan Piotrowski secured a number of extra-focal exposures of ζ Aurigae between 8 P.M. and 5 A.M., E.S.T., on the night of December 14-15 by means of the 16-inch Metcalf refractor of the Oak Ridge Station, but all exposures appear to have recorded the variable already at its minimum light. It seems, therefore, that the ingress must have taken place largely during the daylight hours of the western hemisphere, slightly earlier than expected.

The eclipses of ζ Aurigae are important also for the spectroscopists, since the narrow pencil of light of the early-type component permits one to analyze the stratification of elements in the extended atmosphere surrounding the K5 supergiant more completely than can be done for any other star except our sun. The spectroscopists are, moreover, at an advantage in so far as the crucial phases in which they are interested last much longer than the partial eclipse—a week or so before and after the first and last contact. In point of fact, Thackeray and Beer of the Solar Physics Observatory in Cambridge, England, reported (*H. A. C.*, No. 888) that on February 4.76, 1948, the spectrum of ζ Aurigae still showed strong K lines in absorption, approximately 3.5 A wide, 13 days after the computed emergence of the B star. Their note appears, however, to be so far the only

indication on record to show that the opportunity to observe the rare and fleeting phenomena attending the eclipses of ζ Aurigae has not been entirely missed this time. It will not be until in 1957 that such an eclipse will occur again under equally favorable conditions.

Parallax of SS Cygni: A note of interest to all observers who have followed the vagaries of SS Cygni over the years appears in a recent number of the *Astrophysical Journal*, in which Dr. K. A. Strand, Director of the Dearborn Observatory, gives figures indicating the latest value for the parallax of this star. The result was derived from measures made on plates taken at the Yerkes Observatory. Although the latest value differs radically from that derived by van Maanen in 1938, it is in close agreement with the value suggested by Parenago and Kukarkin in 1934.

The present absolute parallax is 0".32, which places the star at a distance of the order of 100 light years, and gives an absolute magnitude at minimum light of 9.5, or 5.7 when at normal maximum. This indicates that the variable is several magnitudes below the main sequence.

Personal: On March 1, 1918, Leslie C. Peltier made his first observation of a variable star, on R Leonis, 094211. On March 1, 1948, he again observed the same star, and during the intervening thirty years he has contributed 360 consecutive reports, a record unequalled in the ranks of AAVSO observers. Of those who contributed observations in March, 1918, Peltier is the only one still contributing regularly. Congratulations to Dr. Peltier on his magnificent record over these thirty years.

AAVSO Meeting: The Spring Meeting of the AAVSO will be held, at the invitation of Professor Alice H. Farnsworth, on May 21 and 22 at the Payson Williston Observatory of Mount Holyoke College, South Hadley, Massachusetts.

Observations received during February: A total of 3964 observations were received during February from 60 observers, as follows:

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Ahnert, P.	15	35	Garneau	12	18
Ancarani	21	60	Harris	6	6
Bartlett	4	32	Hartmann	133	142
Bicknell	6	41	Hiett	7	10
Blunck	7	7	Howarth	18	18
Bogard	33	59	Kelly	10	15
Brown	4	4	Kirchhoff	17	62
Cain	5	8	Kitley	40	107
Caraioryis	6	12	Kleber	8	50
Chandra	178	264	de Kock	88	244
Chassapis	50	104	LeVaux	15	128
Cilley	27	99	Lodén	5	10
Cragg	128	165	Luft	5	25
Damkoehler	4	10	Mallas	49	319
Darling	5	6	Matthews	13	26
Debono	2	3	Nadeau	76	138
Elias	40	57	Oravec	58	217
Epstein	3	6	Paletsakis	22	139
Escalante	39	145	Parker	20	20
Estremadoyro	5	14	Parks	11	15
Fernald	251	401	Pearcy	5	5
Focas	23	32	Peltier	141	172

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Penhallow	7	11	Smith, B.	34	155
Plybon	5	10	Svolopoulos	20	20
Renner	42	42	Tarbell	4	15
Rich	1	9	Thomas	5	5
Rosebrugh	15	126	Tifft	25	39
Seagroves	5	10	Webb	9	9
Sherman	11	16	Wells	3	3
Sill	20	20	—		—
Slemaker	14	22	60	totals	3964

March 15, 1948.

Comet Notes

By G. VAN BIESBROECK

The numerous comets that were under observation this winter are now reduced to four. All the others are now either too faint or too far from the meridian for further investigation.

In the beginning of the night Periodic Comet Faye (1947 *f*) is still favorably located east of Orion but the magnitude had dropped to 16 on March 5 so that it will soon be lost. It has been followed for nearly nine months at this return.

Periodic Comet Schwassmann-Wachmann No. 1 moves slowly through the constellation of Gemini. No bright outburst has been noticed at this opposition; however the record is incomplete on account of the weather conditions. This object can be followed for a couple months more.

The other two comets are visible in the morning hours: Comet Mrkos (1948 *a*) follows closely the course predicted last month but it loses in brightness more rapidly than expected. The magnitude was estimated here as 13 on March 5 which indicates that soon this object will only be in reach of larger telescopes. The nucleus was well defined but the tail was hardly visible any more.

Comet Bester (1947 *k*) has now emerged from the sun's vicinity. Not until March 5 did the weather here allow a first glance at this bright object. It was found exactly in the position predicted by the ephemeris given last month. Unfortunately haze prevented a close estimation of the brightness. I did not think however that the comet was much brighter than sixth magnitude. Only a faint indication of a tail could be detected. During the coming months this object becomes circumpolar and should remain in reach of small instruments. Cunningham's ephemeris is continued here.

		α h m	δ ° ′	Mag.
1948 April	7	18 50.5	+50 26	5.5
	15	17 14.5	67 28	6.1
	23	14 07.6	73 08	6.8
May	1	11 56.6	68 40	7.5
	9	11 04.4	62 46	8.2
	17	10 42.3	57 46	8.8
	25	10 32.4	+53 46	9.4

The brightness will be found somewhat fainter than the computed value.

The search for the expected PERIODIC COMET FORBES 1929 II should be continued. Cripps' ephemeris shows that it is well placed for observation but it is not expected to become brighter than 13 magnitude.

		α h m	δ ° ′	Mag.
1948 April	7	13 57.0	—11 53	14.6
	15	50.2	40	
	23	42.4	23	
May	1	34.3	11 07	
	9	26.7	10 52	13.9
	17	20.3	42	
	25	13 15.9	—10 42	
June	2	13 13.6	—10 53	
	10	13 13.9	—11 16	13.6

Williams Bay, Wisconsin, March 6, 1948.

General Notes

Dr. Otto Struve of the Yerkes and McDonald Observatories was selected Catherine Wolfe Bruce Gold Medalist of the Astronomical Society of the Pacific for the year 1948, at a meeting of the directors of the Society on December 5, 1947. (*P. A. S. P.*, No. 352, February, 1948.)

Dr. Edwin P. Hubble, Astronomer in the Mount Wilson Observatory, and Hitchcock Lecturer, fall of 1947, in the University of California, spoke on "The Nature of Science" at the University of California, Los Angeles, on the afternoon of March 10, 1948.

The Rittenhouse Astronomical Society, of Philadelphia, held a joint meeting with The Franklin Institute on Wednesday, March 3, 1948. The speaker was Dr. Jason John Nassau, Director of the Warner & Swasey Observatory, Case Institute of Technology, Cleveland, whose topic was "Our Galaxy."

Dr. John A. Hutcheson has been appointed director of the Westinghouse Research Laboratories, succeeding Dr. L. Warrington Chubb. Dr. Chubb is retiring for reasons of health, after 40 years of research with Westinghouse, the last 17 as head of the Laboratories. Last year Dr. Chubb was awarded the nation's outstanding tribute to scientists and engineers, the John Fritz Medal, "for pioneering genius and notable achievements during the long career devoted to the scientific advancement of the production and utilization of electrical energy."

Technical Press Service, Westinghouse Electric Corporation.

Catalog of Technical Books

A new, 1948 catalog of technical books has just been issued by The Chemical Publishing Co., Inc., 26 Court Street, Brooklyn 2, N. Y. This catalog includes the latest books on chemistry, physics, science, technology, petroleum, medicine, foods, formularies, drugs and cosmetics, engineering, metals, technical dictionaries, building construction, etc.

This catalog, conforming with the requests of technical and scientific workers and librarians, gives the date of publication of each book as well as price, number of pages, detailed descriptions, and full table of contents.

A copy of this catalog will be sent free to everyone who is interested in keeping up with the latest technical and scientific progress.

New Satellite to Uranus

Dr. Gerard P. Kuiper reports from the McDonald Observatory, "A new satellite, of the 17th magnitude, was found to the planet Uranus on a plate taken February 15 at the Cassegrain focus of the 82-inch telescope; it was confirmed on two plates taken on March 1. The distance to the planet is about 0.64 of that of Ariel, the innermost of the four previously known satellites, or about 9". Its period of revolution has not yet been determined but may be expected to be about 30 hours. The object could not be seen visually but is readily photographed (exposure time 2 or 3 minutes)."

HARLOW SHAPLEY.

Harvard College Observatory, Announcement Card 890.
1948 March 9.

Australian Gift to Royal Observatory

A gift from the Government of Victoria, a Transit Circle by Troughton and Simms, has arrived in this country from Melbourne and will be installed at the new site of the Royal Observatory at Herstmonceux. The transit circle is an astronomical instrument used for the determination of the positions of the Sun, Moon, planets, and stars, work which has been continuously carried on at the Royal Observatory, Greenwich, since its foundation in 1675.

The instrument will supersede the Airy Transit Circle, which was installed at Greenwich in 1851 and which has reached the end of its useful life. This historic instrument, by which the position of the Greenwich meridian—the zero of longitude—is defined, will be left at Greenwich, along with a collection of more ancient astronomical instruments, to form an historical exhibit for the benefit of visitors to the famous Observatory.

The newly arrived transit circle has an aperture of eight inches and a focal length of twelve feet. It is provided with two graduated circles of three feet diameter, for use in measuring the altitudes of the celestial bodies. It was installed at the Melbourne Observatory in 1884, since which date several improvements have been made to it.

The Melbourne Observatory was recently closed and much of its work has been taken over by the Commonwealth Observatory at Mount Stromlo, near Canberra. Last year, the Astronomer Royal, Sir Harold Spencer Jones, who was visiting Australia, saw the instrument and, when the Government of Victoria was made aware of the requirement for such an instrument at Herstmonceux, it generously decided to make the gift.

The Royal Observatory possesses a more modern transit circle, made by Cooke, Troughton, and Simms, and installed in 1936. Two transit circles are required, however, for the various programmes of meridian observations undertaken at the Observatory.

Telescopic Observation of Bird Migration

Studies in recent years at Louisiana State University have demonstrated the practicability and desirability of making counts at night of migrating birds by use of a small telescope focused on the moon. The number of birds seen in this measurable portion of the sky furnishes an index to the total number passing over a given observation station. The telescopic method also provides a means of computing the direction and altitude of flight. From these data it will be eventually possible to determine to what extent night migrants follow set pathways, whether the flights are continuous in equal volume throughout the hours of darkness, and in what way certain meteorological conditions affect the density and direction of migration. However, large numbers of observations are necessary to obtain results of statistical significance. An intensive program of study in the spring of 1948 is being directed from Louisiana State University. Observation stations ranging from Florida to eastern Mexico and from the Yucatan Peninsula north to southern Michigan are already scheduled for operation. But many more such stations are urgently needed. Any astronomer with access to a spotting scope or a low-powered astronomical telescope who is willing to collaborate in this project, is asked to communicate immediately with George H. Lowery, Museum of Zoology, Louisiana State University, Baton Rouge, Louisiana. Detailed instructions and data sheets will be provided at once so as to permit observations in the full moon periods of April and May.

Book Review

Colloid Science—A Symposium. X + 208 pp. (Chemical Publishing Co., Inc., Brooklyn, New York. 1947. Price $6.00.)

The text of this Symposium was taken from a series of lectures given at Cambridge, England, under the auspices of the Royal Institute of Chemistry. The different sections of this work are presented by the lecturers who are experts in the several fields.

After an introduction by E. K. Rideal the subject of Surface Chemistry and Colloids is given in six sections by A. E. Alexander. This is followed by Thermodynamics and Colloidal Systems by D. D. Eley; The Study of Macromolecules by Ultracentrifuge, Electrophoresis, and Diffusion Measurements by P. Johnson; The Viscosity of Macromolecules in Solution by F. Eirich; The Kinetic Theory of High Elasticity by R. F. Tuckett; Emulsions in Vivo by J. H. Schulman; The Study of Colloidal Systems by X-ray Analysis by M. F. Perutz; Membrane Equilibrium by G. S. Adair; Infra-red Spectra and Colloids by G. B. B. M. Sutherland; and Vingl Polymerization in the Liquid Phase by R. R. Smith.

It can be seen from these numerous subjects that a real attempt has been made to survey the field and present to the reader the many important phases of the subject. Each of the several authors has succeeded quite well in surveying the field assigned to him. The reader will be brought up to date in most instances but not in all cases. In the section on membrane equilibria for example the important recent work of Soellner and his co-workers is completely neglected. This book may be recommended to readers who desire a brief survey of the important phases of Colloid Science.

L. H. Reyerson.